



1969

Philipp G. Frank: Critic of Modern Science

Justin. Synnestvedt
Loyola University Chicago

Recommended Citation

Synnestvedt, Justin., "Philipp G. Frank: Critic of Modern Science" (1969). *Master's Theses*. Paper 2487.
http://ecommons.luc.edu/luc_theses/2487

This Thesis is brought to you for free and open access by the Theses and Dissertations at Loyola eCommons. It has been accepted for inclusion in Master's Theses by an authorized administrator of Loyola eCommons. For more information, please contact ecommons@luc.edu.



This work is licensed under a [Creative Commons Attribution-Noncommercial-No Derivative Works 3.0 License](https://creativecommons.org/licenses/by-nc-nd/3.0/).
Copyright © Justin. Synnestvedt

PHILIPP G. FRANK: CRITIC OF MODERN SCIENCE

by

Justin Synnestvedt

**A Thesis Submitted to the Faculty of the Graduate School
of Loyola University in Partial Fulfillment of
the Requirements for the Degree of
Master of Arts**

November

1969

PREFACE

The purpose of this paper is to present Philipp G. Frank as epitomizing some of the developments that have occurred in the philosophy of science during this century. Frank is particularly well suited as a subject for this study, for several reasons. In the first place, he has been an influential contributor to the growth of modern science, through his work as Professor of Theoretical Physics at Prague, from 1912 to 1938, and as Lecturer on Physics and Mathematics at Harvard, from 1939 until his retirement in 1954. During this time he was a friend and professional associate of such leading scientists, mathematicians and logicians as Einstein, Carnap, Schlick, von Mises, Reichenbach, Bohr, and Bridgman, to name only a few. Die Differentialgleichungen der Mechanik und Physik, which he published in 1925 in collaboration with von Mises, was the bible for students of mathematical physics during the late Twenties and Thirties. He produced a wide range of technical papers from 1904 to the early Thirties, dealing with variational calculus, Fourier series, function spaces, convex bodies, applications to analytical dynamics and hydrodynamics, and the special theory of relativity. In some of these efforts, he joined forces with well-known colleagues, like the mathematicians

Pick and Lowner.¹

In the second place, and perhaps more important for this discussion, Frank was a philosopher too, criticizing and interpreting modern science, and showing an ever-increasing concern with its revolutionary impact on our culture. In this latter role, he demonstrated a unique power of objectivity. Although he was closely allied to the Vienna Circle, and the growing 'new positivism,' he managed to maintain a degree of detachment that permitted him to see this movement in its context, and to present its best ideas without prejudice to the world at large. T. E. Hill has underscored this quality in setting Frank apart from the drift of Logical Positivism, noting that Frank showed an unusual concern for philosophy and the social implications of science.² But it is not just Frank's dual skills as scientist and philosopher which mark him as unusual. It is the role he played of synthesizing the two that proves his leadership in the philosophy of science. In this sense, he personifies what he sought to establish throughout his life - a goal summarized in the title of his last book:

Philosophy of Science: The Link Between Science and Philosophy.

¹Cf. Laszlo Tisza, "Philipp Frank and Physics," in Philipp Frank, 1884-1966, a pamphlet giving "Expressions of Appreciation as arranged in the order given at the memorial meeting for Philipp Frank," Harvard University Memorial Church. (Pamphlet bears no publisher, date of publication, or page numbers.) Oct. 25, 1966.

²Thos. E. Hill, Contemporary Theories of Knowledge, Ronald Press, N. Y., 1961, 444-445.

As a citizen of Europe and America in a period racked by two world wars, Frank felt keenly the disintegrating influences on our society. Yet he never despaired of the power of reason, and of scientific investigation as its principal tool for guiding human conduct. He would not succumb to the pressures of hysteria which cast doubt on the efficacy of science. If science is used for evil ends, he maintained, it is not the scientist qua scientist who is at fault, but the man who starts from faulty goals, and justifies them through false ideologies, and unfounded metaphysics. Now goals are the province of morality and religion, but they are not a world apart from science. Sound education must integrate the two realms. Frank liked to cite Einstein in this regard: "Religion without science is blind; science without religion is lame."³ Frank's own words, spoken to the same Conference on Science, Philosophy and Religion that Einstein was addressing, are similar. He said, "It must, then, be the task of religion, according to the modern conception of science, to do what science is unable to do, that is, set up certain goals for both private and social human life and influence the disposition of human beings in favor of these goals."⁴

Frank argued against the thought that scientists can be,

³A. Einstein, "Science and Religion," in Science, Philosophy and Religion, A Symposium, Conference on Science, Philosophy and Religion, N. Y., 1941, 211.

⁴"Science and Democracy," in op. cit., 228.

or should be, morally neutral. He deplored the cloistered view of scientists who tend to accept the 'metaphysics of their childhood,' without criticism. Scientists have an obligation, as teachers, to point out the metaphysical and moral overtones of their findings, rather than ignoring such problems in the name of objectivity. Indeed, scientists should be particularly well-suited, by reason of their training, to present such questions fairly, and effectively.

Considering the value of this man's ideas, then, it is unfortunate, and not a little surprising, that so little has been written about his life and personality. Perhaps this is because he lived too recently to admit of historical perspective. Another factor may be the relative newness of philosophy of science as a field of study. It was not, for example, until 1931, and then largely due to the prodding of Frank himself, that a Chair of Natural Philosophy was created at Prague, to be filled by R. Carnap. And as late as 1938, when Frank came to America, there was no similar position at Harvard. It may be only after Frank's contemporaries, who joined him in developing this field, have all given over to a new generation, that Frank will be recognized widely, and studied as a great philosopher of science. It is hoped that this paper, if it is of any permanent value, will help to bring publicity to the man whose work it hopes to clarify.

I wish to thank four people who have patiently and thoughtfully solved my problems in this study. Professor Edward A. Maziarz has directed this thesis from the start, using a subtle combination of prodding and encouragement for which I am grateful. It is, however, for the enlightening personal experience of working with him that I am most grateful. Dr. Kenneth J. Thompson, also a faculty representative, has posed many critical questions and comments - always in a constructive manner. His suggestions have effected changes. Rita Brennan has put my drafts together in their final form. This means she has had to decipher my handwriting, as well as correct the many errors of form and mechanics she discovered in the process. Finally, I cheerfully acknowledge a debt to my wife, Sheila, who has not only helped with the typescript, but has at the same time continued to care for three children and a husband in the kind way to which they are accustomed in less hectic days.

J. S.

Glenview, Illinois

CONTENTS

CHAPTER I. PERSONAL HISTORY OF PHILIPP G. FRANK	1
Early life; social and political climate in Europe; kaffeehaus days in Vienna; Frank's informal style; Frank as a scholar; Frank moves to the United States; Frank's interest in philosophy	
CHAPTER II. FRANK AND LOGICAL POSITIVISM	25
Introduction; the setting: A crisis in scientific thought; how to synthesize theory and observation; language analysis in the Vienna group; the Unity of Science movement; social and ethical influences; Frank's divergence from Logical Positivism; disruption by the Second World War	
CHAPTER III. FRANK AS A CRITIC OF METAPHYSICS	57
The metaphysical interpretation of science; metaphysics as common sense; the shift in Frank's views; the chain that links science and philosophy; the break in the chain; the dangers of metaphysics; the value of metaphysics	
CHAPTER IV. MISINTERPRETATIONS OF QUANTUM THEORY	76
Quantum mechanics; the abuse of scientific language; matter and reality; light: Waves or particles?; the Uncertainty Principle; uncertainty and free will; Complementarity	
CHAPTER V. MISINTERPRETATIONS OF RELATIVITY THEORY	97
Relativity in context; the Michelson-Morley experiment; Einstein's Special Theory of Relativity; Einstein's two principles; non-Euclidean geometry and Minkowski space; Relativity or Relativism?; divergent views of Relativity	

CHAPTER VI. SCIENTIFIC METHODOLOGY 119

Introduction; the need for historical perspective; causality: Absolute or pragmatic?; induction and logic in scientific method; validation of theories; acceptance of theories; scientific language

CHAPTER VII. SCIENCE AND CULTURE 150

The divorce of science from culture; science as a balance of mind; scientific specialization; science and materialism; science and Democracy

CHAPTER VIII. CONCLUSION 166

Trends in Frank's thought: Thesis, anti-thesis, synthesis; language analysis: A contribution of modern positivism; the rise and fall of Logical Positivism; Philipp Frank: Pluralist, humanist, relativist

BIBLIOGRAPHY 179

Books by Philipp G. Frank; articles by Philipp G. Frank; commentaries on Philipp G. Frank and his work; works relevant to this study

CHAPTER I

PERSONAL HISTORY OF PHILIPP G. FRANK

1. Early Life

Philipp G. Frank was born in Vienna, March 20, 1884, and died in Cambridge, Massachusetts, July 3, 1966, in his eighty-second year. One can assume that the Frank family, which included three children younger than Philipp, lived comfortably, since Herr Frank was a textile mill owner in a period of rapid industrial growth. Also, the fact that Herr Frank was Jewish suggests that the children experienced a high degree of academic and artistic training. The success of the family is attested to by the fact that Philipp and his brother, Joseph, got international fame in their respective fields - the younger Frank as an architect in Sweden.

That Herr Frank came from Czechoslovakia may in part explain why Philipp chose to live in Prague after the completion of his formal education in Vienna. He would likely have remained there, teaching at the University, had not the Nazi take-over of Europe forced him to come to America in 1938. Of course another reason for choosing Prague was that he had been recommended by Einstein to succeed the latter, who had accepted another position. Frank was appointed Professor of Theoretical Physics at the

German University of Prague, in 1912. At age twenty-eight, he was the youngest professor in the school, and must surely have seen a better chance for development there than he would have expected at Vienna.

Frank never expressed a personal commitment to Judaism, or any other religious creed. Indeed, one of his lifetime acquaintances suggested that he may have been an atheist.¹ The fact that this acquaintance does not know for sure attests to the silence Frank must have kept on the issue. Even so, his humanistic leanings, and, in America, his active role in the Conferences on Science, Philosophy and Religion, added to his Jewish background, give cause for wondering. Certainly he must have been more than normally aware of Judaism, for he exhibited an intimate knowledge of the life of Jews in Prague, and a personal concern for his associates who experienced the Hitlerite regime in Europe. Hopefully, more light on this subject remains to be shed, before all of Frank's personal acquaintances have passed away. It is information that will help to clarify Frank's position with respect to metaphysics and other non-scientific aspects of human endeavor.

2. Social and Political Climate in Europe

Through comments in his biography of Einstein, Frank reveals many of the cultural and political influences that weighed

¹Mme. R. von Mises, letter to Justin Synnestvedt, April 10, 1969.

on Einstein and himself - influences which can account in part for the depth of character and philosophical interests Frank displayed. At the time Frank was completing his doctoral work at Vienna, under Boltzmann, the Hapsburg emperor Franz Josef controlled the large but shaky Austrian Empire including Austria, Hungary, Czechoslovakia, Yugoslavia and parts of Poland, Rumania and Germany. To the north lay the German Empire, under the Hohenzollern Kaizer Wilhelm II, extending roughly from Russia to the Atlantic, including Prussia as its most powerful state.² The whole of these two empires underwent unceasing turmoil which lasted throughout the nineteenth century, especially after 1848, and into the twentieth century, and which led partially to the two world wars. Relative stability did not come until after 1948, and then only because it was imposed by the rule of Russia in the East, and the unifying fear of Russia in the West.

Racial and national prejudices added fuel to the political fires. In 1910, for example, when Einstein was asked to fill the Chair of Theoretical Physics, "There was already," Frank tells us, "a group among the Germans who propagated the idea of the 'master race,' and frowned upon any intercourse with 'inferior races.'³ Since 1888, when political quarrels had caused the Austrian government to divide Prague into two separate universities, there

²Cf. Smith, Muzzey and Lloyd, World History, Ginn and Co., N. Y., 1955, 355-387.

³Einstein: His Life and Times, 80.

had been little connection, and less good will, between the two institutions. Frank continues, "(T)he general attitude of superiority and hostility against the Czechs was quite evident among the German professors and their families."⁴

The irrationality of the situation made a lasting impression on Frank, who often, in later work, used examples from European politics to demonstrate the dangers of non-scientific thinking. "One of the remarkable and frequently comical aspects of this hostility," Frank comments, "was that there was not even the slightest difference between the Germans and the Czechs in Prague so far as race and origin were concerned. The question of which nationality one belonged to was often a question of personal taste and which offered opportunities for earning a living."⁵ Frank gives, as an example, the case of Anton Lampa, who was instrumental in bringing Einstein to Prague. Lampa, a Czech by birth, was just as hostile to the Czechs as were the Germans, and chose to enter the German University in Prague, where he became a student of Ernst Mach.⁶

It is interesting that Frank himself chose to remain in Prague, although his philosophical ideas started in Vienna, and continued to center around the Vienna Circle which he helped to establish there. He later started what might be called the

⁴Ibid., 80.

⁵Ibid., 81.

⁶Ibid., 81.

'Prague segment' of the Vienna Circle. He also managed to institute a Chair of Natural Philosophy at Prague, and to persuade Rudolf Carnap to fill it. Perhaps the geographical isolation from Vienna was to Frank's liking. It is certain that it added to his sense of detachment from the activities of the Circle. Carnap supports this point, speaking of his own days at Prague. "My life in Prague, without the Circle," Carnap tells us, "was more solitary than it had been in Vienna. I used most of my time for concentrated work, especially on the book on logical syntax. By frequent visits I also stayed in close contact with my philosophical friends in Vienna."⁷

Religion was another factor in the turmoil. Frank tells us that Franz Josef refused to confirm Einstein's appointment to Prague, unless the latter should express his connection with a 'recognized church.' Although Einstein had had no formal connections with Judaism since his high school days, he complied with the Emperor's wish, putting his religion down on the appointment forms as "Mosaic," which was the name given to Jews at that time. It was, incidentally, only after he became acquainted with the condition of the Jews during his days at Prague that Einstein became personally committed to a particular religious denomination. His general taste was more for what he called a 'cosmic religion.'

⁷The Philosophy of Rudolf Carnap, ed. P. A. Schilpp, Open Court, 1963, 33.

The position of the Jews in the Prague community was paradoxical. Frank, himself a Jew by birth, notes that "More than half of the German-speaking inhabitants in Prague were Jewish, so that their part among the Germans, who comprised only about five per cent of the total population was extraordinarily important."⁸ To the Czechs, there was no practical difference between Jew and German. Both were to be mistrusted as agents of a foreign power which was apparently bent on driving them into war. Frank clarifies this paradox.

On the other hand, the relation of the Jews to the other Germans had already begun to assume a problematical character. Formerly the German minority in Prague had befriended the Jews as allies against the upward-striving Czechs, but these good relations were breaking down at the time when Einstein was in Prague. When the racial theories and tendencies that later came to be known there as Nazi creed were still almost unknown in Germany itself, they had already become an important influence among the Sudetan Germans. Hence a somewhat paradoxical situation existed for the Germans in Prague. They tried to live on good terms with the Jews so as to have an ally against the Czechs. But they also wanted to be regarded as thoroughly German by the Sudetan Germans, and therefore manifested hostility against the Jews.

Such idiocy within and without the academic community, as Frank here outlines in regard to Einstein, must also have had much influence on Frank himself, who was not only a close friend, but also Einstein's most able interpreter in the intellectual com-

⁸Einstein: His Life and Times, 84.

munity. It was Einstein who had determined that Frank should replace him, when the former left Prague in 1912. There can be no question that Frank was greatly affected by the abuse directed at many of the men whom Frank knew and respected. How much more must have been his horror when his personal friends and he had to flee Europe or be killed by the Nazis. Moritz Schlick was assassinated in 1936, by a fanatical student at Vienna. Ludwig Berwald and Georg Pick were killed in concentration camps. The latter, who was Chairman of the Mathematics Department, had been Einstein's closest colleague before Frank arrived.

3. Kaffeehaus Days in Vienna

Frank received his Ph. D. from Vienna University in 1907. The next few years, until his appointment to Prague, he spent in fruitful research and writing. Having imbibed Einstein's theory of 1905 more thoroughly than any of his contemporaries, he must also have seen its philosophical implications more sharply, for he began immediately to write papers relating to the philosophy of science. He published "Kausalgesetz und Erfahrung" in 1907, "Mechanismus oder Vitalismus?" in 1908, "Gibt es eine absolute Bewegung?" in 1910, and "Das Relativitätsprinzip und die Darstellung der physikalischen Erscheinungen im vierdimensionalen Raum" in 1911. The first paper, on causality, was well received with reservations by Einstein; it was sharply condemned by Lenin. In his characteristic modesty, Frank says of the paper that it

contained some rash overstatements.⁹ He cites, as an example, the thought that causality "can be neither confirmed nor disproved by experience; not, however, because it is a truth known a priori, but because it is a purely conventional definition."¹⁰

During this same period, Frank spent many enjoyable hours each week in informal discussions which led eventually to the founding of the Vienna Circle and Logical Positivism proper. He tells us, in the Introduction to Modern Science and Its Philosophy,

I used to associate with a group of students who assembled every Thursday night in one of the old Viennese coffee houses. We stayed until midnight and even later, discussing problems of science and philosophy. Our interest was spread widely over many fields, but we returned again and again to our central problem: How can we avoid the traditional ambiguity and obscurity of philosophy? How can we bring about the closest possible rapprochement between philosophy and science? By 'science' we did not mean 'natural science' only, but we included always social studies and the humanities.

The broadness of scope indicated here was not just an academic openmindedness. Frank tells us that one of his associates in the group, Otto Neurath, even joined a Theological seminary for a year in order to learn more about the Catholic faith. Theirs was a real living commitment to the advancement

⁹L. N. Ridenour, Review of Modern Science and Its Philosophy, in Saturday Review of Literature, July 30, 1949, 9.

¹⁰Cited in Modern Science and Its Philosophy, Cambridge, Mass., 1949, 54.

of understanding! We shall discuss the Vienna Circle movement below, in Chapter II.

4. Frank's Informal Style

It is important to stress the informality of these coffee house sessions, for it is illustrative of the desire Frank had, as a teacher and student, to make learning a delightful thing instead of a drudge, and to insure that ideas should relate to the students' real experience outside the classroom situation. In this attitude, Frank was advancing techniques that are being studied today in educational psychology. Einstein and Frank held similar views about the motivation to learn. Frank says of Einstein, "The charm of his lectures was due to his unusual naturalness, the avoidance of every rhetorical effect and of all exaggeration, formality, and affectation. He tried to reduce every subject to its simplest logical form and then to present this simplest form artistically and psychologically so that it would lose every semblance of pedantry, and to render it plastic by means of appropriate, striking pictures."¹¹

These same traits were even stronger in Frank's own teaching style, as is evident from the comments made by many of his students and colleagues. In some reminiscences given in honor of Frank, prefacing the 1965 volume of Boston Studies in the

¹¹Einstein: His Life and Times, 90.

Philosophy of Science, Raymond Furth pictures his former teacher and colleague thus.

The first lecture course he gave (in 1912) was on Maxwell's theory of electro-magnetism, and some of us, including Charles Loewner and the late Arthur Winternitz, had selected this course although we were only beginners. At first Frank did not realize that the course would be attended by first year students, who had no knowledge of partial derivatives, differential equations and vectors; but on being told so he took great trouble in helping us to overcome these difficulties.

His style of lecturing was, on the face of it, rather easy going and slow, and he normally devoted the first quarter hour of each lecture to a repetition of the contents of the preceding lecture. But in this way he made us understand the subject thoroughly, in spite of our inexperience, and to arouse our enthusiasm for it. ...

Frank also liked to teach specialized and topical subjects by way of seminars, where the more senior students were given the task of reporting in turn on chapters of books or papers. He did, of course, most of the talking himself, interrupting the students frequently in order to emphasize salient points and to make comments. I especially remember a seminar on relativity theory, Frank's main field of interest at that time. We found these seminars most enjoyable because they were conducted in an informal and friendly atmosphere and brought us in closer personal contact with Frank.¹²

Another of Frank's ex-students, Peter Bergmann, voices the same admiration for Frank's manner, especially effective in the pre-World-War-II decade. Bergmann had been forced to leave his native Berlin in 1933, and took up his further studies at Prague

¹²"Reminiscences of Philipp Frank," in Boston Studies in the Philosophy of Science, Vol. II, ed. Cohen and Wartofsky, Humanities Press, N. Y., 1965, xiii.

because it was "the least inexpensive by a considerable margin."¹³

In this overheated and jittery atmosphere there was one fatherly figure who represented all that was best at the University, Philipp Frank. As Einstein once said of him: "His shirt might show grease spots, but his mind was always in apple-pie order." Philipp Frank never, in the three years I was at his Institute, began a class on time; he might be late anywhere from twenty minutes to over half an hour, and that for a forty-five minute period. But he was full of sparkle, full of stimulus. After class he would drift into the Institute library where Wallauschek and I took care of the collection of books, and he would talk about politics, about physics, about anything that he might have picked up at the "Kaffeehaus." He encouraged all of us students, and he gave us the feeling of a wide-open intellectual window, open to things that happened in and out of physics, and open to things that happened outside the country as well. Philipp Frank saw to it that there was close contact with philosophy of science, presided over by Carnap, with experimental physics at Furth's Institute downstairs, and with pure mathematics, which was taught by Professors Berwald, Lowner and Winternitz.

Time and time again, the trait which seems most to have impressed Frank's students, friends and colleagues, was his ability to accommodate and distill his great erudition into language that could be at once humorous, exact and inspiring. One is reminded of a comment Frank made about Einstein, which is again equally applicable to himself. Frank notes that the latter seemed to look upon life with an attitude of amusement, which came not from any disdain, but from a confidence based on great knowledge that what seems dreadfully important at present will

¹³"Philipp Frank in Prague," in Philipp Frank, 1884-1966.

soon change in the great scheme of things.¹⁴

Frank's acquaintances in America were equally impressed with his humanistic manner as had been his European friends, even after he was old enough, and knowledgeable enough, to warrant a certain degree of dogmatism. Gerald Holton notes his "pursuit of these discussions in a humane and deceptively informal setting, rather than in a stuffy, ex cathedra manner."¹⁵ Similar praises come from another of his Harvard friends, E. C. Kemble.

His was a gentle, unassuming spirit combined with a luminous mind and gifts of simplicity and humor that endeared him to all. He understood the nature of truth and the criteria that must be used to separate truth from mythology. He was a humanist as well as a scientist and philosopher. He understood the predicament of modern man as men who devote themselves to a single discipline cannot; he understood because he was a reader as well as a thinker, a historian as well as a scientist, a conversationalist as well as a writer and lecturer.¹⁶

Let one more citation suffice for now to demonstrate Frank's effect as a man upon his students' personal development. Kurt Sitte recalls that Frank's initial impressions were not

¹⁴Cf. Einstein: His Life and Times.

¹⁵One wonders why Professor Holton used these, and other, identical words to describe Frank on three separate formal occasions, one of them being before Frank's death. First, in his contribution to the Preface, "In Honor of Philipp Frank," of Boston Studies, Vol. II, 1965. Second, in the Memorial Meeting cited above, Note 12, and third, in an article entitled, "In Memory of Philipp Frank," which was signed by Holton, E. C. Kemble, W. V. Quine, S. S. Stevens, and M. G. White, and which appeared in Philosophy of Science 35, March, 1968, 1-5.

¹⁶"Philipp Frank's Years at Harvard," in Philipp Frank, 1884-1966.

always good, especially for the aggressive students.

(W)e thought him lazy at first, until a little of his wisdom, so generously and in good humor shared out to us, had rubbed off to give us the rudiments of maturity. He used to come very late to his lectures, and then to present the cream only while leaving all the dirty details for us to work out. Again in his seminars on the then modern quantum theory he seemed content to learn new material through our poor efforts - though when we got stuck, it was always a sharp remark of his which pointed the way to us. By that time Philipp Frank had become deeply interested in the philosophical problems of science, and this, of course, accounted for what we philistines had deemed his 'laziness.' But we learned from him in this field, too, almost without knowing it. I still recall my amazement when by his words suddenly my eyes were opened to perceive interrelations which I had never even vaguely suspected - and without his help, might not have realized for a long time.¹⁷

Outside as well as in the classroom, Frank was entertaining and friendly. He and his wife, Hania, were welcome guests on social outings, among the students as well as with the faculty. The Franks were forced by economic necessity to lead what might be called a rather bohemian life, aside from their natural proclivity for spontaneity and coffee house geniality. They lived in Frank's office at the Physics Laboratory, during the early part of their marriage. Mrs. Frank, who had little domestic training, was forced to cook meals on a Bunsen burner. Frank tells an amusing story about this period, when Einstein paid them a visit after a number of years absence from Prague. After touring some of their

¹⁷"To Philipp Frank," in Boston Studies in the Philosophy of Science, Vol. II, xxix.

old cafe haunts, Frank and Einstein went back to the apartment for lunch, bearing some meat that Einstein had purchased as a kindness to Mrs. Frank.

We came home (Frank relates) bringing some calf's liver that we had purchased. While my wife began to cook the liver on the gas burner, I sat with Einstein talking about all sorts of things. Suddenly Einstein looked apprehensively at the liver and jumped at my wife: "What are you doing there? Are you boiling the liver in water? You certainly know that the boiling-point of water is too low to be able to fry liver in it. You must use a substance with a higher boiling-point such as butter or fat." My wife had been a college student until then (1921) and knew little about cooking. But Einstein's advice saved the lunch; and we got a source of amusement for all our married life, because whenever 'Einstein's theory' was mentioned, my wife remembered his theory about frying calf's liver.¹⁸

5. Frank as a Scholar

In spite of all this apparent informality and good-naturedness, Frank was a hard worker, and a profound thinker. One reason he was so effective as a teacher is that he knew all the background and ramifications of the problems he was addressing, and could pick out the clearest, most appropriate illustrations, and accommodate them in language suitable to the student. One of his students at Harvard, Jeremy Bernstein, discusses Frank's "almost incredible erudition," and its effect on those who saw it.

¹⁸Einstein: His Life and Times, 171.

He seemed to have read and digested the great philosophical, literary and scientific works in an enormous variety of languages. He once told me that he had studied Arabic, as a young student, in order to be able to read the great texts in that language and, fifty years later, he remembered it sharply enough to be able to write out, which I once saw him do in a discussion with an Iranian student, some of the passages in Arabic that had intrigued him. This vast general culture was also worn instinctively, without pretense, and with the same mastery that characterized his scientific cultivation. If someone pressed him in discussion he would dig one level deeper in his store of knowledge to respond. As a student I had the feeling that what he taught us represented only the pure distillation of a vast reserve beneath.¹⁹

In his work, Frank kept pretty well to himself, not generally teaming up with other teachers or students to solve problems, or to establish research projects. He was cordial, and often shared ideas with his colleagues, but he preferred to think through by himself the problems which most interested him. Apparently he had no taste for experimental work, although he recognized the need for it, and actively supported it. In this regard, he asked Raymond Furth, who was one of his students, to do some research related to problems Frank was working on; and he put the Department's laboratory facilities at the disposal of his young student. Furth says,

I was overjoyed by his offer, and proposed to investigate the phenomenon of critical opalescence

¹⁹"Philipp Frank as a Teacher in America," in Philipp Frank, 1884-1966.

of binary liquid mixtures of which I had read in Perrin's famous book The Atoms. Frank gave his assent, and naively unaware of the difficulties of this problem and my limited experience in experimental work, I started cheerfully to assemble the apparatus under Frank's admiring eyes. ...

For a short time in 1922 the 'black room' (Furth's laboratory) became a centre of attraction for colleagues and friends when, together with my fiancée, who too was one of Frank's students, I had built a radio receiver, and there was great excitement in the evenings, when we were able to pick up fragments of the experimental broadcasts of some European stations. As time went by the 'black room' became more and more crowded with my experimental research students and their equipment, and Frank was always interested in their work although he never took an active part in it.

Nevertheless I suspect that he was not too sorry when, upon my appointment to the Chair of Experimental Physics, in 1931, this part of the activity of his Department came to an end. He was particularly pleased when, on my suggestion, we got permission to exchange the experimental equipment of the theoretical Department with the books and journals of the experimental Department. This arrangement put a very good library at his disposal which was accommodated in the vacated black room (after it had changed its colour!), and from then on his department became once again entirely devoted to theoretical physics.²⁰

After World War I, Frank spent increasing effort on questions of generality and philosophical import, as the titles of his publications indicate, e.g. "Die Bedeutung der physikalischen Erkenntnistheorie Machs für das Geistesleben der Gegenwart" in 1917, and "Wissenschaft und Theologie" in 1920. Peter G. Bergmann

²⁰Boston Studies, Vol. II, xvi.

describes Frank's work of this period.²¹ Frank lectured to seminars at Prague and at the Urania, with other "distinguished visitors" such as Neurath. At the same time, he was finishing the second edition of 'Frank-and-Mises' encyclopedia of mathematical physics. He turned his two-year course in introductory theoretical physics over to Walter Glaser, who later unfortunately became a tool of the Nazis, so that he might concentrate on "frontier subjects," like Dirac's theory of the electron, and the two relativity theories. Bergmann concludes his homage with this summary statement: "Our attitudes toward science and research were formed by the attitude that pervaded all your teaching and writing activities; the quest for understanding, and the search for unifying principles."

6. Frank Moves to the United States

In 1938 Frank came to give a series of lectures on a tour of the United States. That same year Czechoslovakia was sacrificed to the Nazis at the infamous Munich Conference. The subsequent war in Europe prevented Frank from going home. Considering the long time he had watched the build-up of Nazism, Frank could scarcely have been caught unawares. Numbers of his professional associates had already left Europe, especially those of

²¹"Homage to Professor Philipp G. Frank," in Boston Studies, Vol. II, x.

Jewish extraction, like Einstein and Wittgenstein. Nonetheless, Frank was not able to take up his work in America without a hitch. Trying first to get into Chicago University, he found that institution to be already overloaded with refugee professors. After his lecture tour, he and his wife went to Boston. Harvard had no suitable chair for him to fill either, but with the support of Harlow Shapley and Percy Bridgman, a position was made for him temporarily as "research associate in physics and philosophy." Two years later he was given a permanent job at Harvard, with the title of Lecturer on Physics and Mathematics, on a half-time basis - one semester at Harvard, the next away. This arrangement allowed him to teach at other universities around the country, and added to his financial support. Frank filled this post until 1954, when he retired.²²

The same year Frank came to the United States--1938--Carnap and Neurath, who had left Europe earlier, along with the American, Charles Morris, published the first part of their International Encyclopedia of Unified Science, at Chicago. Frank contributed Number 7 of Volume I, Foundations of Physics, which was also published under separate covers in 1946. This essay could be called the culmination of Frank's thoughts in the strict vein of Logical Positivism. After this time, Frank seems to have

²²Cf. E. C. Kemble, "Greetings to Philipp Frank," in Boston Studies, Vol. II, and also E. C. Kemble, "Philipp Frank's Years at Harvard," in Philipp Frank, 1884-1966.

moderated his zeal for the unity of science movement, thus setting himself apart from its more enthusiastic supporters in the Vienna Circle, especially Neurath. Robert Cohen comments on Frank's conception of 'unity of science' in memorial remarks of 1966, asking rhetorically what Frank thought of the "scientific conception of the world."

Did he believe he was part of a revolutionary trend? or if not he, ... perhaps the unity of science movement and the logical empiricist philosophy? ... (Neurath had 'struggled for it;' Carnap was 'touchingly hopeful' of it) The same hope shines through Philipp Frank's papers, particularly in the selection in Modern Science and Its Philosophy. Frank's life work was successful, but he was modest enough and intelligent enough, to see that the unity of science was not a directly public affair. True, it was a public movement of some scientists and philosophers for a decade or two, but I believe Philipp cherished it more as an international seminar and coffee-house. ... Finally, unity of science meant unity of all forms of knowledge and awareness, and it was for Frank a foundation for humane education.

With good friends, he brought the Institute for the Unity of Science alive into the American philosophical world, with publications and with meetings here in Boston and elsewhere. The last major assemblage of the Institute, at which he presided, was its joint meeting with the American Association for the Advancement of Science in Boston in 1953.²³

In certain respects, one might characterize Frank's life in America as his pragmatic period, for it was here that he could

²³"In Memory of Philipp Frank," in Philipp Frank, 1884-1966.

put to practice, in a wide variety of writings and lectures, the theories he had developed in Europe. He began to concern himself more and more with the philosophy of education, and the real effects of science on society. In his little book, Relativity: A Richer Truth,²⁴ he defended scientific relativism as a healthy attitude, and criticized the anti-scientific reactions of a war-conscious public. He wrote criticisms of science teaching, and offered practical solutions to the faults he noted. His own teaching at Harvard became more and more popular, attracting many students from non-scientific curricula to attend - often as many as two hundred fifty per course.

In addition to these strict academic efforts, Frank found time to participate in a variety of organizational work. He became a director of the Conference on Science, Philosophy and Religion, which met annually in New York from 1940 to 1950. He presided for many years over the Philosophy of Science Association. The Institute for the Unity of Science, co-founded by Frank and some of his friends, also had his services as president. He addressed many international congresses in science and philosophy, including, for example, the 12th International Congress of Philosophy in Venice, 1958, at which he gave the opening paper. The Harvard Shop Club on the Science of Science often heard his ideas, as did the frequent Boston Colloquia for the Philosophy of

²⁴Relativity: A Richer Truth, Boston, 1950.

Science, at which he was a regular, and active, attendant. The American Academy of Arts and Sciences held a special conference in 1958 on "Science and the Modern World View," and dedicated it in honor of Frank and Bridgman, who were retiring from active teaching. Similarly, the Proceedings of the 1962-1964 Boston Colloquium bears a preface containing "Greetings to Philipp Frank" from many friends who wished to do him honor.

7. Frank's Interest in Philosophy

As has been said above, and illustrated by the papers he wrote and the organizations he worked in, Frank had a lifelong interest in philosophy. Yet it was not always expressed in the same way, or to the same intensity as in later years. He tells us about an important change in his approach that took place after the end of World War II.

In all my writing before 1947 I had stressed the point that science gives no support to metaphysical interpretations, of whatever type. I had discussed these interpretations only as reflecting the social environment of the philosopher. However, after that time, as a result of my contact with my students and fellow teachers, I became more and more interested in the question of the actual meaning of the metaphysical interpretations of science - idealistic, materialistic, relativistic, and others. For the fact that a great many scientists and philosophers advance such interpretations and cherish them is as firmly established, by our experience, as any fact of physics.²⁵

²⁵Modern Science and Its Philosophy, 52.

Frank's change of view, mentioned here, will be examined more closely below, in Chapter III.

A few more passages are offered here, to complete this general picture of Frank and his usefulness. First, Ernest Nagel discusses philosophy of science, as Frank saw it.

Indeed, he believed that his cardinal aim of obtaining a stable perspective on life can be best achieved through a persistent and self-conscious use of the critical intellectual method of modern natural science. ...

(T)he logical analysis of the language of science, or the articulation of precise rules for assessing evidence in conducting scientific inquiry, are undoubtedly centrally important tasks ... But as he saw it, a philosophy of science that is limited to the discussion of such matters is incomplete; and to be adequate, it must also include certain socio-historical considerations. ... Frank was thus not a purist in his conception of the philosophy of science, and did not hesitate to "thicken" its content even though this disturbed some of his purist friends.²⁶

Joseph Clark, a Jesuit, underscores Frank's difference from the rest of his Vienna Circle companions, in rather emotional terms.

For the violent revisionists of the Wiener Kreis and the pontificating propagandists of the early issues of Erkenntnis did not, I think, deserve the substantial support of our dear departed, gentle, genuine, and generous Professor Frank. He was too good, too gentle, too generous

²⁶"Philipp Frank and the Philosophy of Science," in Philipp Frank, 1884-1966.

and too genuine for their trenchant, indeed, but traumatically truculent company. And he knew it, but never said a word.²⁷

Although these last words seem too honeyed to be serious, they are not out of keeping with the genuine respect and affection which Frank inspired in his many students, colleagues and friends. R. Seeger, of the National Science Foundation, described him as "the physicist's philosopher of physics."²⁸ G. Holton was impressed by his "apparently paradoxical combination of serious power and effortless style."²⁹ E. C. Kemble praised the naturalness of his teaching. "He has not been a victim of the common delusion that ideas expressed in technical jargon are in some way more important and profound than those expressed in plain English. Nor did he ever subscribe to the notion that the difficulty which a lecture course presents to its students is a measure of its value."³⁰ H. Margenau saw Frank as "unique in his generation as a philosopher of science who combines an expert knowledge of modern physics with professional competence in philosophy."³¹

²⁷"Philipp Frank and Metaphysics," in Philipp Frank, 1884-1966.

²⁸Boston Studies, Vol. II, xxvi.

²⁹Ibid., xviii.

³⁰Ibid., xvix.

³¹Ibid., xx.

Finally, Nagel likened him to the admirable carpenter whom Einstein once described in a parabolic story to Frank. He is the one who would rather struggle and struggle to drill a single nice hole in a rotten, knotty board, than to drill dozens of nice holes in an easy board.³²

³²Ibid., xxv.

CHAPTER II

FRANK AND LOGICAL POSITIVISM

1. Introduction

In order that the reader may see Frank better in context, this Chapter is devoted to a consideration of the philosophical environment with which he associated himself, and which he helped to develop over some thirty years. It is a movement which has come to be called Logical Positivism. It should be emphasized at the outset that Logical Positivism is, in fact, a movement - an effort at philosophical growth - rather than a fixed system of thought. It should also be remembered that Logical Positivism is an openended phenomenon, historically speaking, without sharply defined cut-off dates. At one end, it represents a continuation of empirico-positivism, stemming from the thought of men like Hume and Mill in England, and Ernst Mach in Germany. At the other end, Logical Positivism opens to some of the most fruitful themes of contemporary philosophy, especially Linguistic Analysis, which have continued to the present. Nevertheless, we will concern ourselves here only with the main stream of this movement, from about 1910 to about 1940. This represents the growth period of the Vienna Circle - the center of organized Logical Positivism - from its inception to its disruption by World War II.

One wonders where to place Frank in this movement, if he belongs at all. In the first place, the movement contained such a wide variety of personalities and opinions that it is difficult to define it in more than broad-brush outlines. Secondly, as has been suggested above, Frank himself was not a static thinker; he developed within and without the main thrust of the Vienna Circle, while managing always to keep a rather objective detachment from it. Especially with respect to metaphysics, he gradually moved away from the main drift of this group - more so after World War II - when his contact with American teachers and students widened his outlook on philosophy.

2. The Setting: A Crisis in Scientific Thought

Frank presents a short history of Logical Positivism in his book Modern Science and Its Philosophy. There he tells us that the turn of the twentieth century was characterized by feelings of uneasiness over the apparent failure of science (viz. mechanistic science) to give truth, or, in Frank's words "a real understanding of the world." J. C. Maxwell's work in electromagnetic theory, and the experiments of Michelson and Morley in optics had cast doubt on the certainty of Newtonian laws, which had theretofore been accepted as the irrefutable picture of the universe. In mathematics, great changes were brought about during the nineteenth century which added to the confusion. David Hilbert, for example, developed the view that geometry is a purely formal science, and has nothing to say about the real world. The thought

that mathematics has existential content goes back at least as far as the Greeks, yet Hilbert suggested that the connections we normally make between the axioms of geometry and the things of our experience are only conventions; such conventions in no way tell us that geometry is a true picture of reality. In order to establish this idea, Hilbert undertook, successfully, to build a complete formalization of Euclidean geometry. Without changing the system at all, he eliminated every connection between the statements of geometry and those of nature, such as the process of determining congruence which involves superimposing one figure upon another to see if they coincide. Later mathematicians, such as Richard von Mises, were critical of this effort toward purity, which seemed unrealistically extreme. After all, as Frank suggests, if all mathematics is truly tautological - i.e. has nothing to say about reality - it would not be reasonable to apply terms like point and triangle to empirical sciences legitimately, yet this is done regularly with success.¹ There must, Frank and von Mises suggest, be some real connection, in order to yield such fruitful results. At any rate, Hilbert's brilliant work left a broad area of unanswered questions that center around the relation of symbols to reality - one of the primary concerns of the Vienna Circle.

¹"Concerning an Interpretation of Positivism," a review of von Mises' Kleines Lehrbuch des Positivismus, in Isis 33, June, 1942, 683-687.

Albert Einstein, publishing his Special and General theories of Relativity, in 1905 and 1916 respectively, added to the growing skepticism about reality. It was evident that his two Principles, of Relativity and of Constancy, were mutually contradictory, if interpreted in the light of Newtonian mechanics. Yet if this interpretation were disallowed, they led to observable results that held up nicely under test conditions. One of the most spectacular of these tests was Eddington's expedition which discovered the bending of starlight by our sun, exactly in keeping with Einstein's predictions. The question then was: "How are principles of high generality to be interpreted, if at all?" "Is the only interpretation needed the confirmatory results of testing?" This is what Moritz Schlick, Friedrich Waismann, Rudolf Carnap and others of the Vienna Circle were to suggest in their discussions of the criteria for meaning. Waismann's version was, for example, "The meaning of a statement is the method of its verification."² Bridgman added the weight of his work to this view, in a phase of scientific method now termed 'operationalism.'

Max Planck, whose work led to Quantum Theory, initiated still further ramifications on the question of reality. Here, the concern was in the possibility of finding any law at all. Frank characterizes the situation as follows,

²K. W. Britton, in "Logical Positivism," Encyclopedia Britannica, 1966, attributes this statement to Waismann. R. W. Ashby, in "Logical Positivism," in A Critical History of Western Philosophy, ed. D. J. O'Connor, attributes it to Schlick.

The new science of quantum theory gave rise to a repetition of the crisis that had been precipitated about 1905 by the relativity theory, but with even greater intensity. Again it was maintained that scientific method had failed. The new theories do not even claim to give an 'explanation' of the physical phenomena. They claim only to offer mathematical formulas from which the observable phenomena can be derived. ... The argument went mostly that relativity as well as quantum theory gave mathematical patterns without any causal justification.³

Frank says that such thoughts as described above in scientific thought threatened to bring about a violent reaction against science. This revolution was manifest in the tendency of certain thinkers to return to the old metaphysics for answers. To Frank and his associates, raised in an empirical, positivistic environment, this would have seemed a disastrous setback. They examined critically the ideas of their predecessors, and their contemporaries, in the hope of saving science from "going down the drain." We shall look briefly now at some men - Kant, Mach, Boltzmann, Poincare, Rey and Duhem - whose ideas were of major influence on the young Frank and his Viennese friends.

3. How to Synthesize Theory and Observation

In view of the positivists' reaction to metaphysics, mentioned above, it is interesting to find that Immanuel Kant - certainly a metaphysician - had an important effect on the early thinking of Frank and his associates. Kant's influence was both

³Modern Science and Its Philosophy, 46

positive and negative. Frank's group agreed with Kant's powerful presentation of the active role that an observer plays on the knowledge he seeks. Even in the plainest of descriptions, the scientist-observer has certain physical and mental qualities that determine, in a sense, what he discovers. To put it in more contemporary terms, nature can no longer be considered as an isolated and objective thing to be known. Or, as Werner Heisenberg has suggested, in The Physicist's Conception of Nature, which Frank reviewed favorably, "Method and object can no longer be separated."⁴

On the negative side, the Vienna group disagreed with Kant's conviction that the forms of experience are permanent. Neo-Kantians, trying to maintain this permanence, divorced metaphysical principles from science by making them out to be prior. Unfortunately, Frank suggests, this is a fruitless effort. Such a view means, he says,⁵ that the "metaphysical background has no relevance for science proper; it is separated by airtight walls from the domain of scientific discourse. In this way, science became autonomous with respect to metaphysics, but the validity of the metaphysical assertions in the background could not be

⁴Cited in "Man Confronts Himself," Saturday Review of Literature 41, Oct. 25, 1958, 20-21.

⁵Modern Science and Its Philosophy, 24.

checked by any experimental test."⁶

Ernst Mach, who held the chair of Philosophy of Inductive Science at Vienna, from 1895 to 1901, was another intellectual forebear of the Vienna group. In a strongly antimetaphysical approach, he proposed that scientific laws, which claim to give an "explanation" of phenomena are really just shorthand "descriptions" of many events. Theoretical science, then, does not have to be given in terms of mechanics, more than in terms of any other branch of science. This meant, for Frank, that the immanent fall of mechanics - at least Newtonian mechanics - need not drag the rest of science down along with it. Needless to say, Frank's friends were relieved to discover this thought as a possible way out for science.

On the other hand, according to Mach, so-called pure sciences, like mathematics and symbolic logic, are no source of truth either, because they are always true, and tell nothing about the world. Later positivists, differing from Mach, wished to make good use of these tautologous systems, which they considered as their primary tools for relating theory and observation.

⁶Cf. Chapter IX of Modern Science and Its Philosophy, where Frank praises Ernst Cassirer as one who tried successfully to bridge the gap between traditional and modern philosophy in the face of modern science. Yet Cassirer tends to couch his thoughts in terms that are too reminiscent of idealism.

Mach was, in Frank's words, "not the most powerful stimulus to our actual work."⁷ Mach did not give enough credit to mathematics and logical reasoning to suit Frank and his friends. Somehow, a scientist had to be more than a passive collector of facts. Indeed, one had to admit the organizing functions of the human mind; Kant had shown this. Yet Kant was to be avoided as a temptation toward metaphysics.

Mach's successor at Vienna, Ludwig Boltzmann, filled his chairmanship from 1902 to 1906 - the years during which Frank was working on his Doctorate. It is surprising that Boltzmann, under whom Frank worked directly, apparently had little influence, although he was a great thinker. True, Boltzmann was an atomist, rather than a positivist proper, but this did not really set him fundamentally apart.⁸ Boltzmann's apparent lack of influence stems more from the way he said things than from what he said. Frank tells us,

I remember the lectures of a great physicist, Boltzmann, on the philosophy of physics, which I attended as a student. Despite the personal greatness of the lecturer, the effect of the course was slight, because of a lack of a coherent approach. We can notice, on the other hand, that scientists who built their books around a central idea have

⁷Modern Science and Its Philosophy, 7.

⁸In Modern Science and Its Philosophy, 140, Frank says, "In reality the champions of atomism, Maxwell and Boltzmann, were exactly of the same opinion concerning the general nature of a physical theory as Hertz and Mach."

shaped the minds of science students for decades. I mention, just as examples, Mach, Poincare, and Bridgman.⁹

This must have been a good object lesson for Frank, whose great concern with clarity was mentioned above in Chapter I.

What was needed was a bridge between description of facts and general scientific principles. Frank's group discovered part of this bridge in the ideas of Henri Poincare, who admitted the function of mind, but not in the Kantian a prioristic way. For Poincare, scientific laws are conventions that are useful for organizing and predicting events. In themselves, however, they say nothing about facts (and for that reason they cannot be checked by experimentation), nor do they derive from the innate form of the human mind.

Frank compares Mach and Poincare in a single sentence, thus: "According to Mach the general principles of science are abbreviated economical descriptions of observed facts; according to Poincare they are free creations of the human mind which do not tell anything about observed facts."¹⁰ Frank adds, "The attempt to integrate the two concepts into one coherent system was the origin of what was later called logical empiricism."

Abel Rey, a French historian and philosopher of science,

⁹Modern Science and Its Philosophy, 250.

¹⁰Ibid., 12.

seemed to present another element of the sought-for integration. His was the clearest view of the 'new positivism,' as the Vienna group liked to think of it. His book, La Theorie de physique chez les physiciens contemporains, published in 1907, had considerable airing among Frank's associates in the coffee house meetings. In this book, Rey writes,

What was lacking in Comte's or Mill's positivism ... was their ... failure to have established in a new form a theory of categories. Objective experience and mind are functions of each other, imply each other, and exist by virtue of each other. To say that the relations between physical objects derive from the nature of these objects and to say that these relations are constructed by our minds are two artificial theories. ... Our experience is a system, a relation of relations. The relation is the given.¹¹

A third element in integrating the roles of observation and theoretizing, was Pierre Duhem. Duhem emphasized the fact that a structural system tells us nothing about the world of observables. This French Thomist, Frank says, "exerted a strong influence upon our group, and, particularly, upon my own thinking."¹² Part of this influence is evident in Frank's apparent study of, and frequent citations from, the works of Thomas Aquinas. This is certainly not characteristic of the Logical Positivists.

¹¹Op. cit., 392, cited in Modern Science and Its Philosophy, 10.

¹²Modern Science and Its Philosophy, 15.

Duhem pointed out that a scientific theory is a system of propositions, which aim, in the simplest possible way, to explain observables. Because it is a complex system, it will not stand or fall on the basis of one so-called 'crucial experiment.' Also, the fact that physics tries to explain experimental laws through theory indicates the need for a higher level ordering process. In Duhem's opinion, "If the object of physical theories is to explain experimental laws, physical theory is not an autonomous science; it is subordinated to metaphysics."¹³

Duhem represents the neo-Thomist or neo-Aristotelian efforts to incorporate modern science into the older metaphysics. But more often, Frank suggests, the metaphysicians, including both neo-Thomists and neo-Kantians, tended to divorce science and metaphysics, saying that the science was autonomous. Unfortunately, as has been mentioned above, this divorce meant that there was no way to test the validity of the metaphysics by experimentation.

Rather than hold such a dissatisfying truce as just described, certain writers preferred to make a complete break with tradition. These were the real advocates of the new Logical Positivism, of whom the leading representatives were Moritz Schlick, Hans Reichenbach, Ludwig Wittgenstein, and Rudolf Carnap.

Twentieth-century positivism admits concepts, Frank says, that "cannot be deduced from sensory raw material," and this

¹³Ibid., 16.

admission "is exactly the point which distinguishes twentieth-century logical empiricism from nineteenth century 'positivism' of men like Mach."¹⁴ Such concepts, for example, as gravitational field, are developed in the imagination of the scientist, and afterward confirmed by experience. "There is even the question," Frank adds, "whether Mach (himself), if pinned down, would not have agreed that the general conceptions of science are not 'derived' from sensory experience, but constructed by the human imagination to derive observable facts logically from these concepts."¹⁵ Or, as Rudolf Carnap was to say, "The calculus is first constructed floating in the air, so to speak; the construction begins at the top and then adds lower and lower levels. Finally, by the semantical rules, the lowest level is anchored at the solid ground of the observable facts."¹⁶

Whatever differences there may be between the older and newer positivistic schools, however, it is certain that both were agreed on the need, first, to eliminate metaphysics, and second, to make philosophy scientific. This agreement brought about a sense of unity that helps to explain why the movement was able to

¹⁴"Einstein, Mach and Logical Positivism," in Albert Einstein: Philosopher-Scientist, ed. P. A. Schilpp, Chicago, 1949, 281.

¹⁵Ibid.

¹⁶Ibid., 276.

accomplish so much work.¹⁷

4. Language Analysis in the Vienna Group

From the considerations above, one can see how the Vienna Circle movement came to view philosophy as primarily the effort to see what statements mean. Meaningful philosophy can find truth only by the scientific critique of language. In the introduction to Modern Science and Its Philosophy, Frank describes some of the men whose seminal ideas grew into the Vienna Circle. Two of these, Schlick and Reichenbach, whose primary concern was the logical use and metaphysical abuse of language, came from Berlin to make a lasting impression on the coffee house meetings they attended. Later these two came to be leading personalities in the movement.

By way of introduction to the development of the Vienna Circle proper, Frank writes about events in Vienna after he had left to teach at Prague,

At that time (after 1920) Hans Hahn was professor of mathematics at the University of Vienna, /and/ Otto Neurath started working for the City of Vienna, organizing adult education in the social sciences, ... Hahn had started intensive work with advanced students in the field of symbolic logic and the foundations of

¹⁷K. W. Britton, in "Logical Positivism," in Encyclopedia Britannica, 1966, notes some other points of difference between the old and new positivists. He suggests that the Vienna Circle differs from Hume and Mach in placing the basis of knowledge in public experiment - that is, in experiments which many persons can verify - rather than in private personal experience. Here one sees the social criteria of truth, advocated by men like James and Dewey.

mathematics. In 1922, he chose as a basis of their discussions the new book by L. Wittgenstein, Tractatus Logico-Philosophicus. These discussions were the germ of many future developments in the philosophy of science. ...¹⁸

Frank continues,

Hahn became very enthusiastic, starting a close cooperation of the new men with our Viennese group. He envisaged the appointment of M. Schlick as a professor of philosophy at the University of Vienna. He met, of course, a stiff resistance among the adherents of traditional philosophy. But the interest of the scientists in the philosophical background of science has been traditionally high at the University of Vienna. Ernst Mach had owed his appointment to this predilection and Hahn succeeded in enlisting a sufficient number of scientists in a drive to carry through Schlick's appointment in 1922. In this year a close cooperation between Schlick and the old Vienna group began. This common work gained a great deal in intensity and momentum when Schlick persuaded R. Carnap to move to Vienna in 1926.¹⁹

Moritz Schlick was a physicist who had a personal acquaintance with Hilbert, Planck and Einstein. At the same time, he was a philosopher in an environment which afforded the most eclectic view of philosophy in the region. While most German universities maintained their Kantian idealism, Vienna offered studies in a wide range of ideas - Brentano, Descartes, Leibniz, and Hume included.²⁰ For this reason, Schlick was well suited to be the

¹⁸Modern Science and Its Philosophy, 31.

¹⁹Ibid., 32.

²⁰Cf. Frank's "Concerning an Interpretation of Positivism," loc. cit.

'first interpreter of modern science,'²¹ publishing his book Space and Time in Contemporary Physics in 1917.

Ludwig Wittgenstein could be called a spiritual leader of the Vienna Circle, although he did not meet with the group personally, due to his retiring character. Many of Wittgenstein's ideas developed in response to the work of his teacher, Bertrand Russell, who had published, in 1910, the very important Principia Mathematica, collaborating with A. N. Whitehead. Wittgenstein's own work, in a similar vein, Tractatus Logico-Philosophicus, was published in 1922. In this book, Wittgenstein likened philosophical problems to flies trapped in bottles. One has only to see how they get in to know how they can get out again. The problems of traditional philosophy stem from a misunderstanding of language - from making the wrong linguistic turn inadvertently. To put it in Wittgenstein's own words, "When the answer cannot be put into words, neither can the question be put into words. The riddle does not exist. If a question can be framed at all, it is possible to answer it."²²

For the early Wittgenstein, language is a picture of reality. The structure of this language will correspond to the structure of the reality it pictures, if the language is meaning-

²¹Cf. Victor Kraft, The Vienna Circle, transl. A. Pap, Philosophical Library, N. Y., 1953.

²²Tractatus Logico-Philosophicus, 3, cited in J. Hartnack, Wittgenstein and Modern Philosophy, transl. M. Cranston, Doubleday, N. Y., 1965.

ful. Of course the language may become distorted, in which case the resulting ideas will be nonsense.²³

Schlick was in agreement with Wittgenstein in this regard, adding that language is the form through which experience is revealed. This revelation of experience one calls knowledge. Knowledge, which is what is to be communicated, constitutes the form, while experience is the content. Cognition, or understanding, for Schlick, is the process of establishing a 'correspondence' between facts and symbols, or what is the same, between the content and the form of experience.²⁴ To determine the truth of a proposition, one must reduce it to its components, and examine these singly. They are true, in turn, if they can be immediately compared with reality, i.e. with the data of experience. For the empiricists of the Vienna Circle, the simplest possible sentences, upon which scientific discourse can be founded, are those which describe directly sense perceptions like red, warm, deflected to the left, etc. Such statements were called "protocol sentences." But it soon became obvious that there was always, even in such simple protocol statements, an element of subjective interpretation that was bothersome to a strict empiricist. This point remained a source of contention in the Vienna group.

²³Wittgenstein's ideas are summarized in Hartnack's book, Wittgenstein and Modern Philosophy, op. cit.

²⁴Cf. V. Kraft, The Vienna Circle.

Rudolf Carnap decided that it must be a matter of convention where to draw the line in this process of reducing language to sense-data. In Carnap's words, "There are no absolutely basic sentences in the construction of science."²⁵ How far back ought one to go, in establishing the grounds for verification? Schlick suggested that statements which could be used to derive future predictions of events are satisfactorily certain, if they can be confirmed by protocol statements of the form, 'Such and such is occurring here and now.' This idea too was promptly criticized for its apparent subjectivity. Karl Popper, for example, asked how one could hope to establish a scientific law, if all certainty involves the 'here and now' type of statement, which must be repeated over and over for every different experimenter. Discussing this, Victor Kraft, another member of the group, says,

All empirical knowledge consists in the formulation of hypotheses which always go beyond the given, always assert more than the latter, even if they are singular statements. Hypotheses are not verified once and for all by observations antecedent to their formulation, but they always have to be confirmed by subsequent tests. Their verification depends upon correspondence with intersubjectively acceptable observation-reports.²⁶

What was needed was a way of establishing universal statements, such as the laws of science, which, on the one hand, are

²⁵Erkenntnis 3, 209, cited in The Vienna Circle, 119.

²⁶The Vienna Circle, 131.

not exhausted by a 'finite number of cases' of observation, but which, on the other hand, yield predictions about actual future events. Of course this means that there would be no way to prove their validity for all time. It was Carnap who worked to establish the validity of such 'unrestricted universals.' One of Carnap's efforts in this respect was to clarify the meaning of induction, on the basis of probability. This topic will be discussed below, in Chapter VI.

Rudolf Carnap moved to Vienna in 1926, under Schlick's persuasion. He had received his Doctorate at Jena in 1921, after a period of study that was interrupted by World War I. In 1928 Carnap published Der Logische Aufbau der Welt (The Logical Structure of the World). Frank says that the Vienna group considered this book to be the synthesis of Mach and Poincare for which they had been searching. "Carnap," Frank says, "gave the new philosophy its 'classical shape.' He coined many of its terms and phrases and endowed it with a subtlety and simplicity. In the form created by Carnap it became a center of interest and a target of attack on a large scale."²⁷

Perhaps the explanation of Carnap's importance in the Vienna movement is the breadth of his views. He was able to synthesize ideas that had seemed to be at odds, because his attitude was basically pragmatic. Frank notes this in several places. It

²⁷Modern Science and Its Philosophy, 33.

tells us that Frank always recognized the problems attendant upon too pure a theoretical view. It has been noted above that Carnap was willing to admit terms of unrestricted universality, so long as they could lead to observables. Carnap's view is paraphrased by Frank, thus,

The world is to be described by statements that may contain any symbols, provided that from them statements can be logically derived that contain nothing but assertions about similarity between sense impressions. ... When I read this book (Frank continues), it reminded me strongly of William James' pragmatic requirement, that the meaning of any statement is given by its 'cash value,' that is, by what it means as a direction for human behavior. I wrote immediately to Carnap. ... This was as astonishing to him as it had been to me.²⁸

The expression "a direction for human behavior" in the citation above is illustrative of the new direction that Frank's thought was taking as he grew away from the main body of Logical Positivism. This will be developed further below.

Another illustration of Carnap's pragmatism is given in Frank's contribution to The Philosophy of Rudolf Carnap, edited by P. A. Schilpp. Carnap distinguished three components in any situation where language is used: the pragmatic, the semantical, and the logical. In Frank's words, in "any complete theory of language," there must be first "the action, state and environment of the speaker (i.e. pragmatic). ... Secondly, there are the

²⁸Ibid.

words as elements of a certain linguistic system (semantical), and thirdly, the properties of things to which the speaker refers when he uses a certain word (logical)."²⁹

In keeping with their spirit of enthusiasm, which promised the successful employment of scientific method in all areas of study, including the social and psychological, the Vienna group started a concerted push to establish a single language - a 'thing-language' - to bring a greater sense of unity. In a rather extreme effort to keep metaphysics from 'creeping in' to their thinking, they even went through a period of cleaning out all terms that might admit of any sense of subjectivity and thence of idealism. Neurath, especially, who did not share the same background, and thus the same sympathy toward idealistic thought as, say, Schlick and Carnap, jokingly suggested an 'index' of prohibited words, to include uppermost on the list, 'essence,' 'thing,' 'reality,' and 'mind.' This strict view, Frank tells us, which occurred around 1930, was mistakenly viewed by outsiders as a whole new approach. Actually, it was only a 'chapter' in the development of logical empiricism.³⁰

5. The Unity of Science Movement

Frank tells us that Schlick and Carnap expanded the new

²⁹"The Pragmatic Component in Carnap's Elimination of Metaphysics," in op. cit., 160.

³⁰Cf. "Einstein, Mach and Logical Positivism," loc. cit., 276 and Modern Science and Its Philosophy, 35.

positivism into a system - a "general logical basis of human thought." These two came into personal contact with Frank's cronies of the coffee house days, especially Hahn and Neurath, but that his own contacts were limited to vacation periods. Under the unifying leadership of Schlick, the group began to look upon itself as an agency for change, especially through the build-up of a unified science that would embrace all sciences. Frank says,

In 1929, we had the feeling that from the cooperation that was centered in Vienna a definite new type of philosophy had emerged. As every father likes to show photographs of his baby, we were looking for means of communication. We wanted to present our brain child to the world at large, to find out its reaction, and to receive new stimulation.

We decided first to publish a monograph about our movement, next, to arrange a meeting, and eventually to get control of a philosophical journal so that we would have a way of getting the contributions of our group printed.³¹

In 1929, a monograph on the work of the Vienna group was written by Carnap, Hahn and Neurath, to bring their thoughts into public communication. The title of this work alone - Wissenschaftliche Weltanschauung: Der Wiener Kreis (The Vienna Circle: Its Scientific World-Conception) - gives an indication of the global nature of its proponents' views. The name Vienna Circle (Wiener Kreis) was given to the group by Neurath, who felt that it would add a little sales appeal to their work, in the manner of the Vienna Woods, or the Viennese Waltz.

³¹Modern Science and Its Philosophy, 38.

In the same year, in conjunction with a congress of physicists and mathematicians which Frank had organized in the normal course of his office, the Circle decided to work some of their philosophy into the meeting if possible. Accordingly, Frank gave the opening paper, on the subject, "Epistemology of the Exact Sciences." Commenting on this debut, Frank says,

The German Physical Society, which was the official sponsor of this meeting, did not particularly like the idea of combining this serious scientific meeting with such a foolish thing as philosophy. However, I was the chairman of the local committee in Prague, and they could not refuse my serious wish. ... This meeting was to be sponsored by the Ernst Mach Association, which was the legal organization of the Vienna Circle, and the Society for Empirical Philosophy, which was organized in Berlin, and followed in general the line of H. Reichenbach.³²

The paper was well received, in spite of some trepidation on Frank's part. "After the meeting, however," he tells us, "our committee received a great many letters from scientists who expressed their great satisfaction that an attempt has been made toward a coherent world conception without contradictions between science and philosophy."³³

The following year, the first volume of a periodical was published, edited by Carnap and Neurath. This publication, Erkenntnis, became the mouthpiece of the Vienna Circle until the

³²Ibid., 39.

³³Ibid., 41.

War stopped it. Frank cites some of the optimistic lines which Schlick wrote for the first issue, under the title, "The Turn in Philosophy." In that article, Schlick said, "I am justified, on good grounds, in regarding the sterile conflict of systems as settled. Our time, so I claim, possesses already the methods by which any conflict of this kind is rendered superfluous; what matters is only to apply these methods resolutely."³⁴ How ironic are these hopeful words, when one recalls that three years later Hitler rose to power, and three years after that Schlick was murdered!

6. Social and Ethical Influences

One of the most notable aspects of the Vienna Circle movement was its view of morality as a language of meaningless prescriptions. Carnap, for example, held that the specific content of value judgments does not admit of theoretical formulation. In Erkenntnis Vol. II (1931) he says, "The objective validity of a value or a norm cannot (even in the opinion of axiologists) be empirically verified or deduced from empirical propositions; hence it cannot even be meaningfully asserted."³⁵ Similarly, A. J. Ayer, at Oxford - the leading British spokesman for Logical Positivism - maintained that ethical statements are not statements at all, but expressions of emotion, such as approval or disgust, with no

³⁴Cited in ibid., 41.

³⁵Erkenntnis 2, 237, cited in Kraft, The Vienna Circle, 183.

cognitive content.

Other thinkers felt more sympathetic to ethics, and tried to see whether they might not contribute something to the establishment of a meaningful moral discourse. Generally, such efforts have gone no further than discovering what a speaker means when he makes an ethical statement. That is, ethics among the Analysts is, in the more scholarly instances, a survey of popular language usage, and in less scholarly instances, simply an opinion about what a certain word means.

Schlick, like the others, felt that there are no criteria for absolute values. Nonetheless, he thought that ethics is able to validate everyday normative statements, in scientific terms, by comparing them to fundamental norms which are accepted by a particular group. Of course the fundamentals themselves cannot be justified; they are given. Yet, in Schlick's opinion, ethics can explain these norms, from the 'natural laws of behavior.'³⁶

Victor Kraft summarizes Schlick's ethics as follows,

Schlick advocates an ethics of kindness in contrast to an ethics of duty. It is an ethics of the 'beautiful soul' who desires from inclination what the society sets up as a duty. This must be regarded as a goal, to be approached by the process of evolution, whose attainment is far off in the future. In the meantime an ethics of duty alone has practical significance as the past as well as the present prove. ... In my 'theory of value' (Kraft goes on to add) I endeavor to show in detail that hedonism is inadequate for the explanation and justification of values. ...

³⁶Cf. The Vienna Circle, 184.

There are also other equally important sources of value.³⁷

Frank expressed few statements which could be interpreted as indicating a moral system of his own. For the most part, it seems that he remained in the tradition of Logical Positivism with respect to ethics. That is to say, it is reasonable to believe that he looked upon moral injunctions first, as subjective, and not derivable from experience, and secondly, as facts of cultural life which have to be accounted for, and which have a real effect on the behavior of those who hold them. Where Frank probably differs from the Vienna Circle is in the degree of recognition and concern he gives to moral beliefs, especially in their role of directing scientific thought toward good ends. Like Schlick, Frank took moral beliefs as being given, rather than derivable. His interest was more in the reasonable derivation of behavior, from the given moral foundation, than in the justification of the foundation itself. His interest, for example, in the moral reasons for accepting scientific theories (which we shall examine more closely in Chapter VI below) which increased to the end of his life, does not necessarily suggest that Frank had a particular ethical viewpoint of his own. It seems rather to imply Frank's bent for viewing all aspects of any issue, with the dispassionate involvement of a critical judge. Frank's view of morality, then,

³⁷Ibid., 185.

seems to be on the order of a scientific and philosophical investigation of society, rather than a personal commitment.³⁸

This is not to say, of course, that Frank, or for that matter, the rest of the Vienna group, was disinterested in morality. Theirs was not a complete relativism, as might be suspected. After all, the rise of Nazism between the World Wars, with the increasing alarm and disgust it inspired in the Vienna Circle members, must have afforded all of them good cause to entertain strong moral commitments.³⁹

7. Frank's Divergence from the Thrust of Logical Positivism

Frank's differences with the Vienna Circle movement are not so much over specific doctrines as they are a matter of the general approach to science and its meaning. As has been pointed out above, Frank was a thinker of broad scope, and humanistic bent. True, he embraced the primary tenets of the Vienna Circle, namely their interest in science and logic; and their distaste for unfounded metaphysical speculation. But he seems not to have felt the same revolutionary attitude - the same degree of negativism towards older thinkers that was expressed, for example, by the writers of the 1929 monograph, and the early issues of Erkenntnis.

³⁸Ibid.

³⁹For this reason, one can understand the attacks aimed at the Circle, which Frank mentions above, such as the comment in Encyclopedia Britannica, 1966, by K. W. Britton, that the Logical Positivists held to a "general undercutting of all ethics but their own, as being meaningless because unverifiable."

Schlick had said, for example, "All cognition of the being is achieved, in principle, by the methods of the special sciences; every other kind of ontology is empty talk."⁴⁰

Frank does admit to being caught up in the spirit of the "turn in philosophy," which Schlick had described. Thus, Frank writes,

This strong optimistic feeling is psychologically the feeling of a turn. You can ride in a car at high speed and you do not feel anything so long as the velocity remains unchanged. But if a turn or an acceleration takes place, you experience a strong reaction. Today the movement of logical positivism is no longer so conspicuous.⁴¹

The degree of objective detachment - call it historic perspective, perhaps - which is evident in this passage indicates to what degree Frank must have felt himself to be outside the Vienna Circle movement. Of course it is difficult to say whether this is a matter of a scientist's power of observation, which Frank had to a high degree, or truly a feeling of personal estrangement. It seems likely that it shows us a little of both.

Aside from this original feeling of excitement, however, Frank did not view Logical Positivism as the last word to be said in philosophy. Especially after 1940, he tried to synthesize the new and old world views, just as he had tried, before the advent

⁴⁰Modern Science and Its Philosophy, 41.

⁴¹Ibid., 42.

of Schlick, in the days of his coffee house discussions. In Modern Science and Its Philosophy,⁴² for example, Frank takes pains to point out the similarities among the advocates of the older and newer forms of positivism with which he was acquainted. In discussing ideas of meaning and understanding, for instance, he suggests that Wittgenstein, Schlick, Reichenbach, Carnap, and even Mach are "essentially no different" in their ideas. A sufficiently broad interpretation, Frank goes on to say, would also bring Pierce and James into the same language camp with the others. What this brings out is Frank's concern with synthesis, and his recognition that any philosophy which considers itself to be totally new, and totally sufficient, is a naive and flippant effort at best. There is reason to believe that the early members of the Vienna Circle proper had a touch of just this sort of over-confidence.

The courses which Frank taught at Harvard are illustrative of his synthetic approach. He tells us about them, thus,

I now put the greatest emphasis on presenting physics, and science in general, as part of our general pattern of thinking and acting. I presented it on one hand as a logical system that has to be checked by physical experiments and on the other hand as one of the means of expressing man's attitude towards the world, the small world of society and politics and the large world that is our astronomical universe. This more historical approach has been familiar to me since my student years from the meetings with my older friends. All my papers written after 1940 follow this line.⁴³

⁴²Ibid., 32, 33.

⁴³Ibid., 51.

The historical and synthetic viewpoint illustrated by this method of teaching distinguishes its author from the mainstream of Logical Positivism. The same viewpoint is further confirmed when one compares Frank's accounts of the beginnings of the movement with the accounts of other historians, such as R. W. Ashby,⁴⁴ J. O. Urmson,⁴⁵ or Victor Kraft.⁴⁶ These writers indicate, at least indirectly by emphasis, that Logical Positivism began in 1922, primarily through the organizing effort of Schlick. The impression Frank gives, on the other hand, is that Logical Positivism developed out of, and was continuous with, the coffee house meetings that he and his student friends started, way back in 1907. It was Frank, Hahn and Neurath who were, in Frank's words, "the most active and regular members of our group."⁴⁷ Schlick, Reichenbach and Carnap - who are normally associated with the movement - were not even present at that time.

8. Disruption by the Second World War

The Vienna Circle proper was forced to disband suddenly, in 1938, although not to the great surprise of its members, some of

⁴⁴Cf. "Logical Positivism" in A Critical History of Western Thought, ed. D. J. O'Connor, Free Press, N. Y., 1964.

⁴⁵Cf. "Logical Positivism" in A Concise Encyclopedia of Western Philosophy and Philosophers, ed. J. O. Urmson, Hawthorn Press, N. Y., 1960 (article unsigned).

⁴⁶The Vienna Circle, op. cit.

⁴⁷Modern Science and Its Philosophy, 1.

whom had left Europe earlier. Not only the Jews, but most liberal intellectuals as well, felt the pressure of Nazi hatred. Herbert Feigl had gone to the United States in 1931, and Carnap followed in 1936 - the same year Schlick was murdered in Vienna. Waismann and Neurath went to England, while Karl Menger, Kurt Godel, Edgar Zilsel, Felix Kaufmann, Richard von Mises, Reichenbach and Frank came to the United States.

The periodical, Erkenntnis, also called the Journal of Unified Science, was published at the Hague for a few years, but was soon forced to discontinue altogether. The International Encyclopedia of Unified Science, edited by Carnap, Neurath, and the American, Charles Morris, which was published at Chicago in 1938, could be called a continuation of the Erkenntnis work.

The members met with a friendly welcome in the areas they chose for their new homes. In America, for instance, P. W. Bridgman, V. C. Quine, E. Nagel, and Morris helped to bridge the gap between the old world and the new. Nevertheless, it must be said that the movement did not continue in its flower after the War. This, for several reasons. First, the War itself presented a sizable stumbling block for most international intellectual efforts, as the meager number and size of publications during that time can attest to. Secondly, some of the basic theses of the Vienna Circle, such as the 'criterion of verifiability,' were seen by many people to be untenable. Thirdly, other lines of philosophy were taking shape, picking up some of the leads the Circle had pointed out.

Some members inclined toward language, some to mathematics and logic, some to science, and some to social studies.⁴⁸ With this diversity of interests, it is surprising that the group held together as long as it did. Any weakening of the group's original sense of unifying purpose could lead to the scattering of the members. One discovers just such a loss by comparing the Circle's earlier and later views on metaphysics. The primary goal of positivists had always been to do battle with speculative philosophy. For those who came to feel metaphysics had been effectively demolished - for example, A. J. Ayer - there was no need for further destruction. Others, such as Frank, began to see in metaphysics a new interest and meaning, especially in light of the War's moral catastrophes. In either case, the original goal became vague.

Because of the loss of single-minded purpose, the group of men who had accomplished so much together became a group of strong-minded individuals, with valuable and interesting ideas, yes - but with widely diverse opinions. When the original sense of unity died, so did the Vienna Circle. It had not finished its work, yet it had accomplished much. Summing up their contribution, Victor Kraft says,

The nature of logic and mathematics was clarified, the relation of logic to language was even revealed for the first time, and the methods and foundations of

⁴⁸Cf. Danto, A. C., and Harre, R., "The Philosophy of Science," in op. cit., Vol. VI, 289-300.

empirical knowledge were analyzed and clarified with a thoroughness without precedent. It cannot be denied that these accomplishments were accompanied by quite a few oversimplifications and one-side, radical views which have not yet been entirely superceded. ... Imaginative conceptual poetry is surely more interesting for the average person, and the wisdom of a great personality surely has more significance for human life. Yet they are subjective, matters of opinion, unverifiable. Lacking universal validity, they are matters of personal conviction, but do not represent knowledge.⁴⁹

In the remaining Chapters we shall examine what Frank retained from the thoughts developed within the Vienna Circle, and where he went beyond these early developments, especially in his views toward metaphysics, and the sociological aspects of science.

⁴⁹The Vienna Circle, 192.

CHAPTER III

FRANK AS A CRITIC OF METAPHYSICS

1. The Metaphysical Interpretation of Science

It was mentioned above (in Chapter I) that Frank considered his post-War views to be distinctly different from his pre-War views, in respect to metaphysics. Whereas he had early and effectively stressed that science proper does not support one metaphysics over another, he became aware of the need to explain why scientists do, in fact, frequently feel constrained to make such interpretations. Many great scientists - Frank mentions Eddington, Jeans and Whitehead, among others - have chosen to interpret the metaphysical 'meaning' of their discoveries. Although he disagreed with most of these interpretations, Frank came to see them as a fact of life, which was as real as any fact of physics, and which therefore needed examination.

L. N. Ridenour summarized the earlier Frank in his review¹ of Modern Science and Its Philosophy, suggesting that the thesis of the book is to maintain "there are no significant 'questions that are so profound that they cannot be solved by the exact

¹Cf. Saturday Review of Literature 32, July 30, 1949, 9.

sciences.'² Ridenour's paraphrasing is oversimplified. A better picture of Frank's earlier opinion, although perhaps it had changed by the time Modern Science and Its Philosophy was written, is given in Ridenour's words, from the same review, "We can erect a coherent system dealing with all aspects of human knowledge and behavior, by the refinement, extension, and continued application of the methods which have been so successful in the exact sciences." It is doubtful that Frank ever felt the exact sciences, by themselves, could solve all human problems. However, the expression, the "application of the methods" of the exact sciences is sufficiently broad to describe Frank's earlier confidence in science as a way of thought. Even so, it emphasizes too much the autonomy and self-sufficiency of science as a source of truth. Whatever personal confidence Frank may have felt in the methods of science, he was too moderate to use such irrevocable expressions. In this Chapter, as elsewhere, we shall see how Frank fits science and 'scientific methods' into a larger context of human endeavor, which includes, among other things, metaphysics, aesthetics, and religion.

2. Metaphysics as Common Sense

In his book, Modern Science and Its Philosophy, Frank suggests there is a tendency, within science as well as without it,

²It is not clear why Mr. Ridenour puts quotes around the latter half of that sentence. If it is a quote from Frank, which I have not found, it still says nothing about Frank's opinion, since it is an incomplete sentence.

to become so accustomed to scientific principles that one begins to look on them as part of common experience - self-evident, we might even say - and therefore above question, and beyond experimental testing. "I regard metaphysics," Frank says, "as a direct interpretation of scientific principles in terms of the language of everyday life experience."³ There is, of course, a difference between seeing a connection between the principles of science and the ideas of common sense, and considering that the former derive from the latter. It is our natural tendency - perhaps part and parcel with the desire to integrate and generalize our knowledge - to give the supremacy to common-sense interpretations, holding them to be intuitive, and beyond the grasp of experimentation. "This belief," Frank says, "is the very core of the metaphysical interpretation of science."⁴

Two things become evident from this discussion. First, Frank's view of metaphysics as a common-sense interpretation is a rather narrow use of the word metaphysics. We shall return to this point below. The second thing is that Frank seems to imply that such a common-sense approach is inexact, in the best cases, and leads to dead ends in others. The use of common-sense metaphysics, he suggests, is especially frustrated by contemporary developments in science, where theoretical principles seem to have

³Modern Science and Its Philosophy, 290.

⁴Ibid., 289.

retained little or no connection to everyday experience. For example, in the case of such modern theories as relativity, quantum mechanics and the de Broglie wave theory, their only justification is their property of yielding observable facts by means of chains of logical deductions. In these cases, the connection between scientific idea and metaphysical idea is made by a sort of correspondence technique, whose correctness is a matter of opinion. In Frank's words,

Metaphysics attempts a translation of the basic principles of science, but not according to a strictly fixed dictionary; the univocal relation between a term and its translation has been replaced by an analogical relation. But we cannot tell by any exact criterion what is a 'correct' analogy.³

Although these comments, which are in the vein of straight Logical Positivism, seem to disparage metaphysics for being unscientific, that is not to say that Frank feels metaphysics should be scientific. Whatever he thinks of metaphysics, he recognizes that it is distinct from the exact sciences. He points out that the 'truth' of metaphysical statements could not be determined by scientific methods - experimental criteria especially - or there would be no difference between the two fields. If metaphysics is a legitimate branch of knowledge - which view Frank came more and more to support - then its criteria of truth must be decided on

³Ibid., 290.

other than experimental grounds. Frank comments,

In metaphysics, a statement or a system of statements is regarded as 'true' if our common sense understands the validity of the principles immediately without having to draw long chains of conclusions from these principles and without checking some of these conclusions against our observations.

Certainly, men like Einstein and Schrodinger advanced their principles by following some requirements of simplicity or beauty which may also be regarded as requirements of common sense. But they would never claim that the validity of the principles could be proved without checking the conclusions drawn from these principles by physical experiments.⁵

In actual practice, the effort to find a common-sense interpretation for scientific principles consists usually in translating the ideas of current science into the language of an older science that has become so well established - such a common part of everyday thought - that its truths are held to be intuitively obvious. This thesis is developed further in Philosophy of Science - The Link Between Science and Philosophy, which incorporates the ideas Frank developed after writing Modern Science and Its Philosophy. In the later book, Frank continues toward the goal he set in the earlier work, namely, "to break through the wall which has separated science and philosophy for about one and one-half centuries."⁶

⁵Ibid., 296.

⁶Ibid., 6.

3. The Shift in Frank's Views

As Frank himself tells us, his work took a shift in emphasis after 1947, towards a more generous attitude with regard to metaphysics. If we compare some statements from Modern Science and Its Philosophy, which was written in the main before 1947, with other statements from Philosophy of Science - The Link Between Science and Philosophy, which was published in 1957, we shall get a clearer view of this shift.

In addition to the ideas from Frank's earlier work, outlined in the section above, we add the following two statements by way of summary. Frank says, "What we call in a vague way 'common sense' is actually an older system of science which was dropped because new discoveries demanded a new conceptual scheme, a new language of science."⁷ And again, from the final pages of the same work, "The metaphysical interpretation is actually a particular kind of semantical approach; it is a translation into common-sense language."⁸ The tone of these statements is reminiscent of Logical Positivism - the belief that philosophical problems are, at bottom, language problems; one need only express them in more exact terms, and then analyze them by critical logical means, in order to work them out.

In the later book, Frank takes a broader view of the phil-

⁷Ibid., 301.

⁸Ibid., 300.

osophy of science, as well as expressing himself in more general language. The Vienna Circle terminology is no longer evident. For example, in the preface to the book, Frank writes, "In this book we attempt to start from the way in which science is understood by the scientist in his most creative and critical moods."⁹ The term "creative" here is especially indicative of the author's turn toward the subjective elements that characterizes his later views. No longer do his ideas suggest the apparent feeling that one can eliminate as meaningless all psychological and sociological factors that impinge on the scientist's life. Science is not done in vacuo. There is a context for scientific work, which must be accounted for; pure theory - that is, theory that has no connection with human needs - is as impossible as it is useless. Underscoring this, Frank says, "We need to complement the science of physical nature by the science of man."¹⁰ In other words, science is not self-directing. It is a tool whose usefulness is determined by the one who employs it. If science shows, for example, how to destroy, it also shows how to preserve. In Frank's words, "Bluntly speaking, science proper provides us with the technical means by which we can produce the weapons to defeat the enemy, but the philosophic interpretations of science can direct man in such a way that he makes actual use of the weapons."¹¹

⁹Philosophy of Science: The Link Between Science and Philosophy, Prentice Hall, Englewood Cliffs, N. J., 1957, iv.

¹⁰Ibid., xiv.

¹¹Ibid., 19.

It is clear, from all of these examples, that Frank does not consider philosophy to be equated with logic, or reasoning processes. The latter belong to science proper just as much as to philosophy. Rather, he is trying to emphasize the pragmatic, and humanistic, use that science performs. In a sense, this use transcends the scientist qua scientist, placing him in a broader perspective, in relation to all human endeavor. This more humanistic view tries especially to show the common sense meaning of science, not as a prostitution, but as a legitimate way of maintaining goals.¹²

Frank has often been misunderstood, in respect to his thoughts about philosophy and its legitimacy. One example may suffice here to illustrate. Owen Potter, commenting on Frank's paper of 1950 entitled "Metaphysical Interpretations of Science," gives an utterly false impression by quoting Frank out of context. He claims that Frank considers philosophy to be the 'metaphysical interpretation of science.' This view, Potter says, "must partly be ascribed to Frank's conviction that metaphysics is meaningless: 'If we apply to metaphysics the criteria of truth which have been generally accepted in modern science, we can conclude, on good grounds, that tenets of metaphysics ... are neither true nor false, but meaningless.' "It is the purpose of this Note," Potter continues, "to draw attention once again to the fact that the

¹²"Note on Philipp Frank's Interpretation of Science," British Journal for the Philosophy of Science 2: 5, May 1951, 58-60.

criterion of truth by which metaphysics is reduced to meaninglessness is itself metaphysical, in which case Dr. Frank's interpretation must be treated with some reserve."

In response to this criticism, one can do no better than to cite Frank's comments in fuller form, and let the reader see the difference. Frank says,

If we apply to metaphysics the criteria of truth which have been accepted generally in modern science, we can conclude, on good grounds, that tenets of metaphysics (e.g. reality of the external world, mental character of the universe, etc.) are neither true nor false, but meaningless. Although this argument can hardly be refuted, the interest in metaphysics has abated very little. It is claimed that the concept of truth which has been accepted by science does not provide the only valuable kind of truth. Metaphysics may be meaningless for the scientist 'as a scientist', but may be of the highest value for human life.¹³

Frank goes on, in this article, to give a brief history of the relation between metaphysics and science. He points out that many notable scientists favor metaphysics. Planck, for example, was one who "believed strongly in a metaphysical interpretation of science."¹⁴ Frank suggests that there is a two-fold practical use for metaphysics - as an interpretation of science into common sense terms, and as a guide for human conduct. He also notes that there have been two ways of reuniting science and philosophy.

¹³"Metaphysical Interpretations of Science," British Journal for the Philosophy of Science 1: 1, May, 1950, 60.

¹⁴Ibid., 62.

Whitehead, Sartre, Heidegger and Bergson represent the effort to build a new metaphysics. The positivists, on the other hand, try to join metaphysics and science by scientific method and language. Finally, Frank concludes, "If we regard metaphysics, as is suggested in this paper, as a short cut between science and common sense, we acquire a new perspective of human thought in the past and the present."¹⁵ Clearly there is no one-sided positivism in these remarks.

The metaphysician might wish to look upon the subject of his studies as somehow more elevated than Frank's common-sensical view suggests it is. The metaphysician seeks to find necessary truth, as opposed to the temporal and contingent sense-oriented ideas of science. Yet it must be admitted that metaphysics ought somehow to relate to human experience, or it is, in fact, meaningless. It is just this relation to experience which Frank points out so nicely, as being primarily commonsensical or intuitive.

Metaphysical discussions start from ideas that are meant to be self-evident. Yet the very fact that such discussions seldom resolve conflict suggests that self-evidence is not so easy to attain. It cannot lie solely in observable empirical data, which are the grounding of science, nor can it lie in the axioms of logic or mathematics, which are purely formal, and therefore unrelated to reality. What is left, but common sense, or intuition?

¹⁵Ibid., 90.

Of course this foundation is unsatisfyingly vague and difficult to define. But Frank is generous enough to point out that the foundations of science, too, and even mathematical systems like geometry, are likewise vague.¹⁶

4. The Chain that Links Science and Philosophy

Frank says there is a natural chain that links science to philosophy - a chain that has been broken and needs reestablishment. One end of this chain is anchored, as it were, to direct observation of facts; the other to generalities called 'intelligible principles.' Frank describes the chain, thus,

We have along the chain statements of various degrees of generality. On the one hand, statements of fact; on the other, general principles that are clear and intelligible in themselves. Between these, we have statements of intermediate generality - Ohm's laws, Newton's law of gravitation, the laws of electrodynamics, Mendel's laws of heredity - not intelligible by themselves, but useful theories.

This distinction is obviously connected with the double criterion for belief. If we have statements of intermediate generality - laws of physics, for instance - why do we believe that they are true? In science we use the criterion of truth, which requires that we can derive from these laws facts which are in agreement with experience. We say that the law is confirmed by experience. As we have mentioned, it is false to say that these laws of intermediate generality are ever 'proved' by experiment, or worse, that they can be 'derived from the facts.' One can derive a statement only from a more general statement, never from one which is less general.¹⁷

¹⁶Cf., for example, "Geometry, An Example of a Science," Chapter III in Philosophy of Science: The Link Between Science and Philosophy.

¹⁷Philosophy of Science, 22.

This last sentence is of primary importance, It goes directly against the popular conception of positivism, and ties Frank into a broader tradition that includes Aristotle, Aquinas, and Kant, among others, whom he studied at length, and frequently cites in support of his ideas. One must add, however, that Frank does not suggest Positivism is guilty of the view that general theories derive strictly from observations. He notes that even Comte knew that one must start from theories to observe order. In fact, Comte accepted the need for religious principles to get the ball rolling.¹⁸ General statements are, Frank says, "a product of the human mind; this process may be called induction, inductive guessing, imagination. In any case, it is not logical derivation."¹⁹

As we noted above, one end of the philosophy-science chain is based on 'facts'. Frank notes, first of all, that what the scientist actually observes are such things as scintillations on a counter, deflections of a needle, and liquid levels in a tube.²⁰ What the scientist infers from these observations about the nature of the underlying causes is another thing altogether, and is open to question. There is always a certain mental element in science,

¹⁸Cf. "Einstein, Mach and Logical Positivism," in Albert Einstein: Philosopher-Scientist, ed. P. A. Schilpp, Tudor, N. Y., 1951.

¹⁹Philosophy of Science, 22.

²⁰Ibid., 6.

even at the most basic level. Kant tried to show that one never has a direct knowledge of 'reality', even though he himself believed that Newtonian science was true - in fact, necessarily true. What one observes, Kant suggested, is determined by the innate ordering capability of the human mind - not by the thing-in-itself which is the underlying cause of our sense-experience. The latter is beyond the reach of human knowledge.

Another point that Frank frequently makes against the 'pure empiricists' is this: If the best scientific theory were the one which should confirm all observations, then a simple catalogue of data - a list of observables - would constitute the most perfect science.²¹ But clearly such a view would be no science at all. Theories of this form would be as complex as what they purported to explain; they would be useless. There must be an element of simplicity in science. This idea will be considered again in Chapter VI, under the topic of acceptance of theories.

5. The Break in the Chain

Modern science has come more and more to disregard the question of 'reality', calling it an irrelevant metaphysical consideration. Frank comments on this, as follows:

The other end of the chain comes from the longing to know 'why'. Science does not tell us 'why'; it only answers questions concerning what happens, not 'why' it happens. This longing to find out 'why' is nothing more than the longing to derive scientific statements from general principles that are plausible and intelligible. Such a longing stems from the belief that there

²¹Ibid., 42.

are such principles. There have been, of course, a great many opinions about the criteria for what is plausible and intelligible.²²

How was the science-philosophy chain ruptured? In ancient and medieval times, science was an off-shoot of philosophy. Thus, for example, a philosopher such as Aristotle might say that all matter seeks its natural place, which is down, so that bodies fall, while fire, which is immaterial, seeks its natural place in heaven, and thus rises. Or one might believe, for another example, that the most perfect motion, because the simplest, is a circle, so the planets, which are perfect, must describe circular orbits.

With this ancient kind of 'science', there was little concern with testing hypotheses by careful measurement. Not that careful measurement was beyond the reach of scientists. Rather, measurement and data gathering was thought to be the work of artisans and mechanics - beneath the station of philosopher-scientists who constructed theories. In this regard, Frank cites A. N. Whitehead, thus,

All the world over (Whitehead says) and at all times there have been practical men, absorbed in irreducible and stubborn facts; all the world over and at all times there have been men of philosophic temperament, who have been absorbed in the weaving of general principles.²³

²²Ibid., 23.

²³Science and the Modern World, Macmillan, 1925, cited in Philosophy of Science, 25.

Frank expands this idea of Whitehead, in these words,

We might say that the 'lower' strata collected facts while the 'higher-ups' advanced principles. Contact between the two types of knowledge was discouraged by social custom. If a man of high social status attempted to apply his 'philosophy' or 'science' to technical problems, he was severely criticized. Experimental testing of general principles requires manual labor, which was regarded by the ancient Greeks as the appropriate occupation of slaves but not of free men.²⁴

However, the people at the technical end became interested in deriving their own theories to explain their observations. They were unwilling to take a back place with respect to the philosophers. Frank continues,

From about the year 1600, however, science became more pretentious; it wanted to derive practical mechanics from theoretical mechanics. Then the chain split in the middle. From the principles of intermediate generality, the physical laws, observed facts could be derived. "Scientists" were no longer interested whether the physical laws could be derived from principles of high generality.²⁵

Because they have discovered that science can proceed directly, for the most part, from these principles of intermediate generality to observables, many scientists have come to feel that it is useless to bother looking for so-called 'first principles'. Yet, as Frank points out, there is always the hope to find the

²⁴Philosophy of Science, 26.

²⁵Ibid., 28.

most universal law - the overall generality - that explains the world. "The dream of science is to derive all facts from one principle."²⁶

From the purely scientific point of view, one may not care whether or not the principles one uses are 'intelligible', but only whether from them one can derive observables, by means of suitable semantical rules, or 'operational definitions', as they are commonly called. However, Frank warns, "By taking this general scientific point of view, we have disregarded a large part of our chain." This philosophical part of the chain involves statements that cannot be checked by direct observation, but which should not, nevertheless, be disregarded. "Such statements have just as practical results as the scientific ones; they have a direct effect on human behavior."²⁷

6. The Dangers of Metaphysics

There is a danger in the desire to interpret scientific statements by means of philosophical ones, even though the latter seem to come closer to common sense. In Chapters IV and V below, we shall discuss some of the abuses that this effort has piled onto modern science. While it is true, as Frank says, that "in a way, philosophy is nearer to common sense than science," and that "the more science had advanced into the theoretical field, the

²⁶Ibid., 42.

²⁷Ibid., 37.

more remote from common sense its general principles have become,"²⁸ science must often free itself from its metaphysical context in order to advance. "It is a matter of fact," Frank notes, "that the advance in science has consisted to a large extent in the replacement of the common-sense world by a world of abstract symbols." He continues, "If we want to formulate general principles from which a wide range of observable facts can be derived, we must discard the language of common sense, and make use of a more abstract terminology."²⁹

Metaphysical systems can retard science by claiming that new ideas are foreign to what is accepted as true in common experience. One can see the groundlessness of this view, if one considers that scientific theories which at one time were considered to be revolutionary, from the philosophical or common-sensical viewpoint, have become so well established by use that they come to be looked on themselves as common-sensical. Then they, in turn, are held up as criteria by which to judge newer theories. Newton's laws, for instance, are widely regarded today as intuitively obvious. However, at the time when organismic cosmology was popular, Newton's laws were considered to be totally contrary to intuition. Similarly, it is the apparent self-evidence of Newton's laws today which makes it difficult for one to feel comfortable about con-

²⁸Ibid., 44.

²⁹Ibid., 45.

temporary scientific theories like relativity and quantum mechanics.

Bearing these things in mind, one can appreciate why Frank seems to view metaphysics as a mixed blessing. On the negative side, it causes sluggishness in pure science; on the positive side, it helps to direct and stabilize the work that science does. Whatever may be the personal tastes of a particular scientist for metaphysics, metaphysics is a fact of human life, and tells something about human nature. For this reason, no dignified scientist can fail to take note of it.

7. The Value of Metaphysics

Frank does not make an explicit judgment about the value of the two-fold science-metaphysics chain of human endeavor that he describes. Instead, he clarifies it, in the following summary statement,

The scientific way (via mathematical derivation and experimental verification) is often a very long one. Therefore man requires a way by which these principles become directly plausible; this means a way by which they can be connected with common sense by a 'short circuit' ... Philosophy introduces into science something in which the scientist 'as a scientist' has no interest. As a matter of fact, the scientist is also human and has his weaknesses, if one may call this requiring that the general principles of science be plausible in themselves a weakness.³⁰

³⁰Ibid., 47.

In this Chapter, it has been shown in what way Frank's views on metaphysics and common sense have developed after the Second World War. One may say that they represent a liberalizing of the strictly positivistic faith in science alone to show the way to human advancement, and that metaphysics, because it is beyond direct confirmation, is 'meaningless' at best, and harmful at worst. This is not to say, by any stretch of the imagination, that Frank became some sort of mystic, or even romantic. He never gives the impression that intuitive thinking is to be exalted above the methodology of experimental science. However, it does show us how Frank as a philosopher increased in scope, and to that degree how he came much closer to a true picture of the part science plays in human endeavors generally. The following chapters undertake to examine what is perhaps Frank's greatest forte: his ability to see through the metaphysical misinterpretations of scientific theory, to the real meaning and nature of science itself.

CHAPTER IV

MISINTERPRETATIONS OF QUANTUM THEORY

1. Quantum Mechanics

Although quantum mechanics postdates relativity theory by two decades, it developed from work which was done in electromagnetic theory before the end of the nineteenth century. Maxwell's theories to describe electromagnetic phenomena were originally expected to support Newtonian mechanics, although he employed a wholly different mathematical technique. Maxwell himself tried to link his theories to Newtonian models. But it was soon discovered that the two areas failed to jibe well with respect to experimental evidence. H. Hertz said outright that there was no sense in trying to justify Maxwell's equations in terms of other theories; the laws of electrodynamics are Maxwell's equations.¹ Frank points this out as one of the breaking points of the strong belief in Newtonian science. He adds that there was a period during which a dual interpretation was allowed - a mechanical world, explained by Newton's laws, and a world of ether phenomena, including Maxwell's descriptions of electro-magnetism. But this could not last.

¹Cf. Philosophy of Science: The Link Between Science and Philosophy, 130.

In 1890, J. J. Thomson showed that a particle of very small mechanical mass might possess a tremendous inertia, if its speed or electrical charge were sufficiently large. "Later on," Frank continues, "one ventured the hypothesis that there might not be any real mass at all, and that inertia was a phenomenon of the electromagnetic field. From this hypothesis the great Dutch physicist, Hendrik A. Lorentz, derived that the apparent mass of a particle increases with its speed and increases beyond all limits if the speed approaches the speed of light."²

This idea, that the apparent mass of a particle seemed to change, was an upsetting idea for those who were completely sold on the validity of Newtonian mechanics - especially those who gave the latter the support of common-sensical or even metaphysical necessity. Yet the theories indicating such a mass change led to experimental derivatives more in accord with observation than those which held for the constancy of mass. This does not mean that it is true to say mass changes, and false to say it does not. Physical theories do not speak of truth, in the sense of demonstrating the nature of 'reality'. Frank puts it this way,

The correct way of describing the situation is approximately as follows: The operational definition of mass which has been used in Newtonian mechanics loses its usefulness and must be dropped. In order to keep up the continuity of physical science, we introduce

²Ibid., 131.

again a term 'mass,' which is defined by an operational definition which cannot be identical with the definition of mass used in Newton's mechanics.³

Besides the problems of very small masses changing, quantum theory also involves a view of the motion of small bodies that is called the 'uncertainty principle', which was first given a formulation by Werner Heisenberg, in 1927.⁴ He maintained that the product of the uncertainties for coordinate and momentum of a particle is a constant:

$$\Delta x \cdot \Delta p = h.$$

We shall discuss this theory further below. However, for the present, we would note that the constant (h) in the formulation above is the same constant that Neils Bohr discovered in relation to the permissible orbits of electrons in the so-called Bohr atom,⁵ and the same constant that enters into the quantized picture of black-body radiation, and the photo-electric effect studied by Philipp Lenard,⁶ in which light appears to radiate in packets of discrete energy (E), of the form: $E = hf$ (where 'f' is the light frequency). The constant (h) bears the name Planck's Constant,

³Ibid., 146.

⁴Cf. Zeitschrift fur Physik 43, 1927.

⁵Cf. Philosophical Magazine 26, 1913, in three papers (Frank's reference in Philosophy of Science, 373, Chapter 8, Note 18).

⁶Cf. Annalen der Physik 8, 1902 for Lenard's experiment (Frank's reference in Philosophy of Science, 373, Chapter 8, Note 14).

after Max Planck, who proposed what Frank calls "the most revolutionary hypothesis of the twentieth century".⁷ It was Planck who suggested, in 1901, the 'quantum hypothesis', namely, that there is a minimum quantity of energy.⁸

Bohr himself developed an idea similar to the uncertainty principle, which is somewhat broader in application. Noting that one can set up experimental conditions in order to determine either the position of a particle, or its momentum, but not both in the same experiment, Bohr generalized Heisenberg's principle thus: "Evidence obtained under different experimental conditions cannot be comprehended within a single picture, but must be regarded as complementary in the sense that only the totality of the phenomena exhausts the possible information about the objects."⁹

2. The Abuse of Scientific Language

The problems of language abuse is one that has been of paramount concern, among the positivists of the Vienna Circle, and more recently among the so-called Analysts, especially in England and America. Wittgenstein had early suggested that most philosophical problems are pseudo-problems which would disappear as soon

⁷Philosophy of Science, 153.

⁸Cf. Annalen der Physik 4, 1901, 553 (Frank's reference in Philosophy of Science, 370, Chapter 6, Note 5).

⁹Cited in Albert Einstein: Philosopher-Scientist, ed. Schilpp, 220.

as their origin was discovered to lie in the inappropriate use of words. Following this up, Neurath and Carnap pushed for the employment of a 'physicalist' language to eliminate confusion and to unify all the sciences, including the social. Carnap, for example, held that the confusion that has surrounded the meaning of quantum theory "always arises from the material mode of speech." One such point of confusion occurred, for instance, in J. J. Thomson's discussions about electron charges, which included the terms "real mass" and "apparent mass". Frank suggests, "(T)he misinterpretation can therefore be avoided only if one tries to set up a direct short circuit between the physical principle and the moral principle,"¹⁰ which might be done through the use of a consistent 'physicalist' language.

Frank discusses the tendency - an "unfulfilled longing", he calls it - to bring back the old unity between science and philosophy.

Through the work of Galileo and Newton, anthropomorphic medieval physics was expelled from conscious intellectual life. There remained, however, an unfulfilled longing to bring about the unity of animate and inanimate nature which had been present in medieval physics but was missing in the newer physics. There was left only one problem, for which no satisfactory solution could be envisaged: to understand the processes of life in terms of physics. For that was the necessary condition for a unified conception of phy-

¹⁰Modern Science and Its Philosophy, 161.

sics, which had fitted in so well with the vitalist conception of life.¹¹

One manifestation of this longing to unify physical and animate nature is the effort to see in quantum mechanics certain proofs of the underlying spirituality of nature. It is important to note at the same time that Frank does not recognize any corresponding effort to develop a purely materialistic metaphysics. That is, if materialism is considered as "the belief that all processes of nature can be reduced to the laws of Newtonian mechanics, then this is not a philosophic principle, but a physical hypothesis."¹²

If science is to advance, and avoid the linguistic pitfalls of traditional metaphysics, it must maintain the most exact and unambiguous terminology possible. To this end, "A 'soulless' psychology and a 'matterless' physics have been established as parts of 'Unified Science'. Words like 'matter' and 'mind' are left to the language of everyday life where they have their legitimate place and are understood by the famous 'man in the street' unambiguously."¹³

3. Matter and Reality

If, as experiments confirm, mass can seem to disappear,

¹¹Ibid., 159.

¹²Ibid., 160.

¹³Cf. "Man Confronts Himself," Saturday Review of Literature 41, October 25, 1958, 20-21.

does this not indicate that the materialistic view of nature is false? Does one not see strong evidence that reality is immaterial - that it is energy and spirit? Frank says no. He warns that such anti-materialistic interpretations are not from science, but from what he calls the "humanized or popularized formulation of these results."¹³

Frank discusses several men who hold to the views that he criticizes here. Bernard Bavink, for instance, wrote a book in 1933 whose title alone, Natural Science on the Path to Religion, suggests the view in question. Another physicist, J. H. Jeans, has put forth similar ideas, in his books, The Mysterious Universe, and The New Background of Science. He suggests that "Today there is a wide measure of agreement, which on the physical side of science approaches almost to unanimity, that the stream of knowledge is heading towards a nonmechanical reality; the universe begins to look more like a great thought than like a great machine. Mind no longer appears as an accidental intruder into the realm of matter."¹⁴ Still another physicist, A. S. Eddington, has voiced comparable opinions, in his book, The Nature of the Physical World.

These men all seem to feel that the twentieth century has destroyed the mechanistic model of the world in favor of a mathematical, and therefore a more human, model. But a clear look at the historical development of the old mechanics will show the

¹⁴Cited in Modern Science and Its Philosophy, 129.

error of this belief. Frank reminds his readers frequently that Newton's laws are also a mathematical system. It is only because they deal with events that are close to everyday experience that they have received such a high degree of acceptance. These laws certainly were never, nor can they be, "read off" the events they claim to describe. Newton's second law, for example, which states that a body's acceleration is proportional to the force acting on it, and inversely proportional to its mass ($F = ma$), does not follow automatically from one's observations of tides, falling bodies, and celestial orbits. even though it does predict these events well. Both Leibniz and Descartes studied the same universe that Newton studied, yet their mathematical descriptions of motion were different from Newton's formulation.

It should be noted, also, that a mathematical mechanics has not always been thought to be closer to spirituality, or Divine Omniscience, than non-mathematical mechanics. Copernicus' censure by the Catholic Church is a case in point. His system of celestial motions was upheld as being 'mathematically true', at the same time as it was condemned for being 'philosophically absurd'.¹⁵ Frank calls the distinction between mathematical and mechanical models "inappropriate". He says,

We know today that the motions of bodies with velocities comparable to that of light can be des-

¹⁵Cf. "Metaphysical Interpretations of Science," op. cit., Chapter III, Note 13.

cribed only with the help of the relativity theory of Einstein, the motions of the smallest particles in the atoms only with the help of quantum and wave mechanics. ... If we understand by mechanics the doctrine of the motion of 'bodies of average size with moderate velocities,' then we can rightly say that modern physics has established the impossibility of a mechanical basis for the processes of nature. If we say, however, that the mechanical foundation has been replaced by a mathematical one, it is, in my opinion, a very inappropriate mode of expression. We ought to say, rather, that the place of a special mathematical theory, that of Newton, has been taken by more general theories, the relativity and quantum theories.¹⁶

4. Light: Waves or Particles?

From the time of Newton, there has been a dispute over the nature of light. Some held that light was a wave phenomenon, whereby the propagations were due to disturbances moving in a medium, like ripples moving through a liquid. Huyghens developed the idea of interference and reinforcement between waves, to explain the common optical phenomenon of interference patterns. If two waves have their crests 'lined up', the light will be intensified or reinforced, while if the crest of one wave coincides with the trough of another, the waves will interfere.

Although this wave picture was adequate to explain many of the common optical phenomena, it had some basic set-backs. For example, all attempts to discover what the 'medium' is in which light waves travel have failed. If this medium, called 'ether' exists, some very sophisticated experiments have not been able to

¹⁶Modern Science and Its Philosophy, 131.

detect it. One of the most conclusive in the series of such tests was the famous Michelson-Morley experiment of 1881. In Chapter V this experiment will be described in some detail. For now, we simply point out that the failure to discover ether might be explained by assuming that the ether alters the very instruments which are used to detect it, in such a way that it remains undiscoverable. That is, if, in accordance with Maxwell's theory of electro-magnetic radiation, all instruments undergo certain forces from light that tend to contract them in the direction of light propagation, they will yield one and the same reading for two different events. This contraction hypothesis which was suggested, and formulated mathematically by Lorentz, has been strengthened by experiment, although it was originally suggested only as a very unlikely explanation for the negative results of Michelson and Morley's work. In any case, the relativity theory, which will be described below, assumes that whatever ether may be, if anything, it cannot be discovered.

Another problem which the wave theory faces is the explanation of so-called 'black-body radiation'. The wave hypothesis would suggest that a light source emits light uniformly in all directions in a spherical wave that loses its intensity as the inverse square of the distance from the source. One might expect thus to find that the intensity of light energy falling upon a detector, such as a screen, would diminish along a continuum, and that no matter how far the detector were placed from the source,

some light energy would always be found falling on it. However, this is not the case, experimentally. It is discovered that the intensity of detected light does not fall off smoothly and continuously, but by discrete increments, so that at a sufficiently great distance from the source, the detector records only distinct bursts of intermittent energy, with a certain discrete, least amount of intensity, or else none at all. This experiment, carried out by Lenard in 1902, was interpreted to mean that light is emitted in small packages, called photons, rather than in a continuous undulation. The discreteness of these packages is the origin of the term 'quantum', or 'definite amount'.

It is obvious that the wave model, unaltered, cannot account for this phenomenon. Yet it is equally clear that the wave theory is useful for most optical phenomena, which Newton's laws for particle motion cannot explain. Frank tells us of Einstein's efforts to keep the most fruitful aspects of the wave idea. "In order to alter the undulation theory as little as possible, Einstein assumed that light should remain a propagation of waves, now electromagnetic waves, but that the energy should not be distributed evenly in the wave homogeneously. There should be condensation of energy that will propagate like parcels in such a way that the screen can never be hit by less than one parcel."¹⁷

Another area in which the quantum idea was confirmed deals

¹⁷Ibid., 199.

with the orbits of electrons around the nuclei of atoms. According to classical mechanical analysis, one should observe orbits with a range of energies that is theoretically infinite. Yet spectral analysis shows that atoms actually have only a limited number of orbits, with very definite energy levels that occur over and over. In hydrogen, which is the simplest atom, Bohr discovered that only those orbits are found whose angular momentum is an integral multiple of $h/2\pi$, (where h is Planck's Constant, described above, and which also enters into the discrete energy levels found in the radiation of light from black-bodies.)

It is well to remark again that this quantized view of orbiting electrons is only a mathematical model which can lead to appropriate observational results. No one knows for sure that particular orbits are actually excluded, since it is not certain that electrons are really orbiting, in the sense that planets orbit the sun. Indeed, it is not even certain that electrons may legitimately be considered as little pieces of charge, nor that they act the same in atoms as they do in radiation, or in a conductor.

Noting these discrepancies with respect to light phenomena, and the failure of the wave theory to explain them, Louis de Broglie tried to justify, or modify, Newtonian mechanics in order to account for all the known observables. He decided, in effect, to view all such phenomena, and also those of particle motion on the larger scale, as part of the same general behavior. This behavior

can be described, according to de Broglie, by wave mathematics, which involves second-order differential equations.

Frank tells us,

To make this generalization, de Broglie assumed that a type of wave could be introduced (called by him "matter waves," later Broglie waves) that accounts for the trajectories of material particles by a theory of undulation accounts for the paths of light rays ... De Broglie's hypothesis was simply that a particle's motion was determined by a radiation, the photons of which had the same momentum as the particle. This means that the wave length λ of this radiation is determined by:

$$P = mv = h\nu/c = h/\lambda, \text{ or, } \lambda = h/mv,$$

known as 'de Broglie's equation.' De Broglie's law for the motion of particles then was: If small particles of mass m and the speed v are moving through slits in the diaphragm or around obstacles, they behave like photons of a wave length h/mv , the 'de Broglie wave length.'¹⁸

In these examples, there seems to be little justification in holding that light, or for that matter, particles either, are wave phenomena any more than they are particle phenomena, following the common-sense use of those terms. One does not really know what the underlying nature of these events is; but one can use theoretical models to describe them. The models which Newton employed are close to one's daily experience, because they describe things that are close to our daily experience. The de Broglie wave theory is far from satisfying to common sense, yet it seems

¹⁸Philosophy of Science, 204-205.

more adequate than the older theory for deriving confirmatory experiment, about events which are far from one's daily experience. It should be noted that the de Broglie 'waves' are not three-dimensional waves; they are a mathematical picture. By the same token, one must look critically at the idea that wave theory supports metaphysical idealism. As Frank states, rather bluntly, "It is hardly more plausible to regard beauty and mystical communion with God as de Broglie waves than to regard them as material masses. All the mental entities, beauty, religious experience, etc., are no more a part of quantum mechanics than they are of Newtonian physics."¹⁹

5. The Uncertainty Principle

As has been stated above, it is found, in confirmation of de Broglie's hypothesis, that beams of small particles undergo the same sort of scattering and diffraction phenomena as light beams undergo. That is, if one beams electrons, for example, through a slit onto a screen, some electrons will be bent out of their straight path to form a series of light and dark bands or fringes to the sides of a central bright band that lies directly in line with the beam's path before it passes the slit. The distribution of these bands will follow the typical 'bell curve' that is common in probability theory. This is to say that some of the electrons will apparently receive a component of momentum perpendicular to

¹⁹Ibid., 239.

the original path, when they interact with the slit. If one narrows down the slit, thus hoping to define the position of the beam more precisely, this sideways component of momentum will increase proportionately. There is no way to determine the final path of any single electron. However, with a large number of electrons, it is possible to determine with great accuracy the final distribution of bright and dark bands on the target screen.

From the wave equation, it is found that the uncertainty of original position, that is, in this case, the width of the slit, is related to the sideways component of momentum imparted by the slit, by this equation:

$$\Delta x \Delta p = h$$

This principle, which states that there is no way experimentally to determine simultaneously the exact position and momentum of a particle, bears the name Heisenberg Uncertainty Principle.

Frank explains the principle in Foundations of Physics.

We must always decide in what result we are interested: in the position of the particle passing the diaphragm, or in the momentum of this particle. In each case we can make a prediction. In the first case we can predict the diffraction pattern on the screen. In the second case we know the momentum which the diaphragm gets from the particle, and we can make predictions by means of Newton's mechanics. We can predict the motion of bodies which are hit by the diaphragm.²⁰

²⁰Foundations of Physics, Vol. I, No. 7, of Encyclopedia of Unified Science, Chicago, 1938, 473.

Again, in the same article, Frank continues,

If we consider a great number of particles passing the slit of the width Δx , most of them have a momentum $P_x = h/\Delta x$ parallel to the screen. But also all smaller and greater momenta will occur with a certain frequency according to the Gaussian distribution of errors.

6. Uncertainty and Free Will

As a result of these experimental and mathematical findings, interpreted by metaphysically oriented persons, it has been suggested that there exists, in the physical universe as in the mental universe, a built-in indeterminacy, that is akin to 'free will'. Eddington, for example, in his book The Nature of the Physical World, 1928, suggested that only after 1927 (the year Heisenberg proposed his Uncertainty Principle) was it possible for thoughtful scientists to feel justified in their religious beliefs.²¹ In a later book, The Philosophy of Physical Science (1949), Eddington denied that one can support the belief in free will by Heisenberg's principle.²² Nevertheless, according to Frank, it is Eddington's earlier idea that has remained popular. "The philosophers and theologians who advocate this belief," Frank tells us, "are not interested in advocating indeterminism in physics, but in claiming

²¹The Nature of the Physical World, 350, cited in Philosophy of Science, 254. Cf. also Frank's review of Heisenberg's The Physicist's Conception of Nature, under the heading, "Man Confronts Himself," loc. cit.

²²Cf. Philosophy of Science, 255.

that there are events and phenomena that follow laws that are different from the physical laws."²³

Arguing against the 'free will' interpretation of the Heisenberg principle, Frank uses the thoughts of Nalin K. Brahma, a Hindu philosopher, who is favorable to metaphysics, but who realizes its inappropriateness in this context. Brahma says,

If future experiments reveal to us that the indeterminism supposed to exist in the movements of the electron is really nonexistent, philosophy would find itself helpless to prove its position if it now accepts the argument of Professor Eddington ... Freedom and other metaphysical truths cannot be proved in the sphere of phenomena where space, time and causality are the only categories that rule.²⁴

The ideas of Eddington's earlier book are echoed by another British physicist, James Jeans, in his book of 1943, Physics and Philosophy. In Jeans' opinion, "The classical physics seemed to bolt and bar the door leading to all freedom of will; the new physics hardly does this; it almost seems to suggest that the door may be unlocked if we could only find the handle. The old physics showed us a universe which looked more like a prison."²⁵

At an early period, Frank warned against such overstatements as Jeans makes here. In the Encyclopedia of Unified Science, for instance, he wrote,

²³Ibid.

²⁴Cited in ibid.

²⁵Cited in Philosophy of Science, 240.

We must carefully avoid the misunderstanding which has been caused by the way some physicists have discussed the relation of uncertainty. One hears sometimes the statement: "It is impossible to measure simultaneously the position and the momentum of a small particle." This sounds as if there would be small particles which possess certain positions and certain momenta. We are told that we can measure either of them but that nature is so diabolic as to prevent us from measuring both simultaneously. This statement is rather misleading. The expression "a particle with a certain position and a certain momentum" has no operational meaning if de Broglie's hypothesis is accepted.²⁶

Twenty years later, Frank had much the same thing to say,

These laws for the behavior of particles are, of course, very different from the Newtonian laws and very different from our common-sense ideas about particles. As Bohr pointed out, we must avoid ascribing to an atomic object (such as an electron) traditional properties of a particle. As we have learned, "position and velocity of a particle" is an expression without operational meaning if applied to small particles.²⁷

7. Complementarity

In keeping with the findings about indeterminacy, as outlined above, Neils Bohr put forth a more general idea that applies to other fields of knowledge in science. In a paper entitled "Discussions with Einstein on Epistemological Problems in Atomic Physics," Bohr said, "Evidence obtained under different experimental conditions cannot be comprehended within a single picture, but

²⁶Op. cit., 474

²⁷Philosophy of Science, 243.

must be regarded as complementary, in the sense that only the totality of the phenomena exhausts the possible information about the objects."²⁸

Frank has put the principle of complementarity thus,

Under certain circumstances, the language of 'positions of particles' or 'point events' must be used; in other circumstances, excluding the ones above mentioned, we speak of 'momenta of particles' or 'impulse events.' If we make use of all possible information about the present state of the world, we must use both languages. Then we can predict all events which our actual science enables us to predict.²⁹

A similar idea was proposed by Heisenberg in The Physicist's Conception of Nature. There he suggests that whereas nineteenth-century scientists looked for knowledge of the world as something lawlike and permanent apart from the knower, modern science is characterized by a more "modest" view, in which the role of the scientist is examined too. One no longer looks for atoms 'in themselves' but as the scientist experiences them. In other words, "Method and object can no longer be separated."³⁰

The point these authors make, and Frank agrees, is that the scientist's knowledge of the world depends in large measure upon the language he uses to describe it, especially with respect to the operational definitions he gives in order that the theories

²⁸Cited in Philosophy of Science, 220.

²⁹Foundations of Physics, 476.

³⁰Cf. Frank's review, "Man Confronts Himself," loc. cit.

may be tested. Metaphysical misinterpretations, Frank says over and over again, stem from the misuse and confusion of the language which describes scientific work. In one of his last publications, in 1958, Frank put the idea in these words: "The new physics does not teach us anything about 'matter' and 'spirit', but much about semantics."³¹ Similarly, in another paper of the same year, he said: "I believe that the advances in philosophy which have been stimulated by twentieth-century physics, like relativity and quantum theory, are not advances in metaphysics, but advances in semantics."³²

Robert Oppenheimer expressed a certain disagreement with Frank in relation to the origin and meaning of complementarity, which is well to point out, in closing this Chapter. Oppenheimer said that whereas Frank feels that science seems to lead the way toward cultural advancement, while philosophical interpretations follow up with their support, it must be admitted that Bohr's complementarity idea did not develop in this way. In fact, it did not come from science at all, but from Bohr's rather metaphysical leanings. Complementarity, according to Oppenheimer, sprang from Bohr's "early interests in the complementary character of the introspective and the behavioral description of man, in the complementary character of dealing with experience in the light of

³¹"The Present Role of Science," The Humanist 19, 1959, 8.

³²"Contemporary Science and the Contemporary World View," Daedalus 87, 1958, 63.

love and in the light of justice, and from the ... tensions of comprehending in one description causal explanation of behavior and moral condemnation of behavior."³³

³³Cf. "The Growth of Science and the Structure of Culture: Comments on Dr. Frank's Paper," Daedalus 87, 1958, 67-76.

CHAPTER V

MISINTERPRETATIONS OF RELATIVITY THEORY

1. Relativity in Context

As has been pointed out above, Frank demonstrated a particular ability for placing the advances of science in their historical perspective. This is especially true with respect to relativity theory, because Frank was a student and personal friend of Einstein, from the time of the latter's first impact upon the scientific world, in 1905 - the year he published his Special Theory of Relativity. As was pointed out in Chapter I above, Frank was the man whom Einstein chose to replace him at Prague.

Relativity theory developed as an effort to synthesize older theories that seemed at first to be incompatible - theories which had been presented in order to explain separate areas of observable phenomena. Specifically, Newton's laws for the motion of particles, which had such great success in their proper realm of middle-sized objects, and moderate velocities, failed to explain experimental results when they were applied to the behavior of light. Light phenomena were observed to uphold, instead, Newton's Theorem of Relativity. In effect, this law says that it is impossible to determine the motion of a reference system, such as the world, by examining the motion of bodies relative to this

system, if the system in question is not undergoing acceleration. The same holds for the motion of light, as will be explained below.

Newton's Theorem of Relativity is described by Frank, thus,

The positive formulation is: By knowing the relative initial condition of masses in a vehicle, we can predict their future relative motion without knowing the speed q of the vehicle itself. The negative formulation says: By observing the motions relative to a vehicle, we cannot find out the constant speed of this vehicle, provided it is moving in a straight line relative to an inertial system. We can also say: a vehicle moving with uniform motion relative to the inertial system (S) is itself an inertial system that may be called (S'). From these considerations it clearly follows that the speed q of (S') relative to the inertial system, or, according to Newton to absolute space cannot be derived from any physical experiment. This speed q has no operational meaning in physics and was given a meaning by Newton within the system of theology.¹

2. The Michelson-Morley Experiment

The Michelson-Morley experiment of 1881 was an effort to confirm the presence of a medium, called ether, in which light might travel as a wave disturbance. Such a medium had long been assumed, since light phenomena had been derivable geometrically if they were looked upon as the propagation of waves. Since Huyghens, at least, the wave explanation jibed well with the optical experiments known, such as image formation, and diffraction patterns.

The experiment was based on reasoning that goes as follows: If there is an ether, which is fixed, or absolute with respect to

¹Philosophy of Science: The Link Between Science and Philosophy, 126.

bodies, then the earth will travel through the ether, and any light propagation which originates on the earth will feel, as it were, the effect of an 'ether wind'. If the light is travelling with the ether, it will be speeded up by the ether. If it is travelling perpendicular to the ether, it will be pushed aside, but not affected in the direction of its propagation. Analyzed algebraically, two identical light rays, travelling through the ether at right angles to each other might thus be expected to take different times to travel the same measured distance on the earth's surface.

Keeping this analysis in mind, the two experimenters, Michelson and Morley, built an apparatus which sent two beams of light out at right angles to each other. The beams were reflected by a series of mirrors and half-mirrors back to a single finish point which was a telescope. The two paths were made identical in length and optical qualities, to the best standards available. It was expected, from principles of interference, that if either beam were slowed down in its path more than the other beam travelling at right angles to it, one should expect to see destructive interference, to the extent that the beams were out of phase. The whole apparatus was mounted on a heavy stone table which could be rotated. In this way, either beam could be directed in any sense relative to the points of the compass, to insure that they might be oriented appropriately to the ether wind, wherever it might be produced. Also, the rotation of the table could allow one to

check on the length of the two light paths. If one path were longer, through an error in the apparatus, light would obviously take a longer time to traverse it than the second path, regardless of ether wind. But by rotating the table, one would observe the same inequity in the new position.

When the experiment was carried out, no change was noticed that could be called interference, at any position of the apparatus, at any time of year. This lack of result could easily have been explained simply by assuming that ether does not exist; yet the belief in ether was so well entrenched that other explanations were offered instead. These reasons are summarized in J. A. Coleman's little book, Relativity for the Layman,² which may be paraphrased as follows.

First, the earth might be fixed in the ether, with everything else moving relative to the earth and the ether. Of course it was not seriously considered that the earth should occupy such a unique position.

Second, an ether wind would not appear, if the earth dragged the ether along with it, but this suggestion had problems, too. Ether dragged along with the earth would drag incoming starlight with it, causing us to observe all light to come from the same direction, which clearly is not the case. Also, studies of the Fresnel drag coefficient indicated that the earth could not manage

²Op. cit., 37-41.

a total drag, which was required to explain this phenomenon.

Third, if the light velocity was always constant with respect to its source, the velocity would vary with respect to the ether. The ether would not be detected, since the two light beams would have the same velocity relative to the interferometer. That the light might vary with respect to the ether, of course, went against the whole tradition of wave motion in media, and, for that matter, made the whole subject almost irrelevant. Also, astronomical observations, especially those concerning double stars, made it highly unlikely that light velocity depends on the velocity of its source.

Fourth, and now most acceptable was an explanation concocted almost as a logical exercise, to explain the experimental results. At first it was hardly thought feasible. In 1893, Fitzgerald suggested that all objects moving through ether contract in the direction of their motion. This contraction would occur in whichever arm of the interferometer was parallel to the motion of the instrument, relative to the ether. This means that the light travelling along that arm would go on a shorter path, but because it would be slowed down proportionately, also, the effect could not be seen. The two beams - one travelling at normal speed over the normal distance, and the other travelling at a slower pace, over a foreshortened distance - would arrive at the finish point simultaneously. There would be no interference pattern to see. Although this hypothesis - called the Fitzgerald-

Lorentz contraction hypothesis - seemed fantastic at first, it received considerable support in 1895, when Lorentz presented his theory about the electron composition of matter. Einstein's work of 1905 added further to the feasibility of this hypothesis.

In Frank's words, the Michelson experiment "showed that Newton's theorem of relativity holds also for the phenomena of light propagation in moving vehicles, although according to Newtonian mechanics and optics this should not be the case. Einstein ventured, therefore, the hypothesis that the principle of relativity might be a principle of higher generality than Newton's laws of motion and the ether theory of light."³

3. Einstein's Special Theory of Relativity

Frank describes the climate which these findings produced - the climate in which Einstein proposed the Special Theory of 1905. Frank says,

During a certain period, physicists gave a 'dualistic' presentation of their science. One part was regarded as 'physics of matter;' mechanics, acoustics, heat; the other part, the 'physics of the ether' contained electricity, magnetism, and optics. Very soon it became apparent that such a clear-cut division did not yield a satisfactory derivation of all experiences about the interaction between the motion of material bodies and electromagnetic wave propagation.⁴

Very soon after the Michelson-Morley results were published,

³Philosophy of Science, 134.

⁴Ibid., 131.

starting with Hertz in 1889, physicists began to drop the idea that all phenomena should be reducible to Newtonian mechanics, because the latter simply could not account for all the observables. The thought of dropping Newton's system was not a comfortable one; it went counter to generations of thought. As Frank says,

Maxwell's equations for the electromagnetic field and Lorentz's hypothesis about the distribution of electric charges in 'material' particles were accepted only because the observed facts about the motion of bodies and the propagation of light could be derived. Thomas Aquinas' criterion for the 'inferior' type of truth, the 'scientific', not the 'philosophical' truth, became the decisive criterion.⁵

Einstein's goal, Frank tells us, was to set up principles from which the interaction between light propagation and the motion of material particles could be derived, in the simplest possible way, without recourse to the ether hypothesis, which had proved so fruitless, or to Lorentz's ideas of the distribution of electric charges in bodies.⁶ It should be noted that the principle of simplicity, about which more will be said in Chapter VI, is one criterion for choosing a certain hypothesis over another, especially if a theory is to have a high degree of generality. It was Einstein's hope to find a most general Field Theory which could be used to derive all the phenomena of electromagnetism, mechanics,

⁵Ibid., 133.

⁶Cf. Philosophy of Science, 134.

and gravity. If he had not had a deep conviction of the unity of the universe, in a philosophical sense, it is doubtful that he would have pushed his idea as far as he did, or that he would have influenced the work of contemporary science so greatly.

4. Einstein's Two Principles

From observations about the ether theory, Einstein generalized the Principle of Constancy. This can be stated as follows:

There is a system of reference (F) in the world, with respect to which light is propagated through vacuum with a constant speed c , whatever the speed of the source of light with respect to (F) may be.

Secondly, from the laws of motion, Einstein took the Principle of Relativity, which can be stated as follows:

A vehicle system (F) may move with a constant speed along a straight line with respect to (F'). We start any optical or mechanical experiment with given initial conditions relative to (F'). Then our principle says that the outcome of the experiment is not dependent upon q , or, in other words, if the initial conditions relative to (F') are given, the further motion and light propagation with respect to (F') are determined; they do not depend upon q .⁷

Immediately, one might well object that the last principle seems to lead to absurdities. If the two systems (S) and (S') are moving relative to each other, say, with a velocity of v , and light travels with a constant velocity c with respect to S, its velocity with respect to S' must be $c-v$, or $c+v$, depending whether

⁷Ibid.

S is moving faster or slower than S'. To say that the velocity of light is c with respect to both systems is to say that $c-v$, (or $c+v$) is equal to c , while v is not zero. This is impossible, mathematically speaking, if not also logically contradictory. But it must be remembered that the argument against relativity assumes that one can measure c with respect to both S and S' at the same time. Actually, this is impossible; our measurements are all operationally defined in their relation to earth (S). Since one cannot check this principle directly by observation, it is safe from refutation. That is not to say the theory is in some sense metaphysical, more than any other physical hypothesis which claims to be more general than a list of simple observations. The only refutation of any theory is that it fails to lead to observables; the only proof of any theory is that it can be confirmed by the observables derived from it. It must be noted that Einstein's principles have led to hypotheses that do jibe with experimental results, and which have very practical consequences in the kind of physics which goes beyond daily experience, such as nuclear reactions, and space travel.⁸

5. Non-Euclidian Geometry and Minkowski Space

It can safely be said that one of the prerequisites for the advent of modern physics was a certain softening up of the principles of the older science. The developments in measuring the

⁸Cf. Foundations of Physics, loc. cit.

speed of light, and the search for ether worked to help prepare the ground. Maxwell's equations also helped to break down the belief that theories must be intuitively obvious. In a similar way, the developments in logic and pure mathematics during the nineteenth century greatly influenced contemporary views of epistemology, and the relations between theory and observation. Among the most important mathematical developments in this respect are those of non-Euclidean geometry, especially the work of Lobachevsky, Hilbert and Minkowski.

According to the ideas of relativity theory, rigid bodies cease to have their normal rigidity in certain circumstances where high speeds are involved. Thus, for example, yardsticks and clocks will be respectively slowed down, and shortened, along the direction of their motion, but not in the direction perpendicular to their motion. This idea was proposed in order to explain the apparent constancy of light velocity, in all reference systems; it cannot be proven directly, as has frequently been stated.

Rotating bodies such as phonograph turn-tables, or the Milky Way Galaxy, must be distorted, since the peripheral speed of such bodies is greater than that close to their center of rotation. Of course no point on the phonograph turn-table ever reaches relativistic speeds, so for all practical purposes, it remains rigid in the normal sense of the term. But in the case of our Galaxy there might be noticeable variations from Euclidean geometry. Consider, for example, a large triangle whose vertex points away from the center of rotation, and whose base is near the center.

Because of its relativistic motion, the vertex angle would be less than it would be at rest, while the other two angles would remain almost unaltered, since they move little. In effect, this would produce a 'defect' in the sum of the interior angles; they would add up to less than 180 degrees. It would also tend to curve the lines forming the vertex inward, so that the vertex would appear to be closer to the base than it really was. Of course there seems to be no way to determine where the vertex 'really is'.

In a long chapter from Philosophy of Science: The Link Between Science and Philosophy, on "Geometry as an Example of a Science," Frank discusses the relation of Euclidean geometry to physical things.⁹ His main argument is that mathematical geometry in its purest form has nothing to do with 'reality'. It is an axiomatic system of an 'if, then' character, which is consistent within itself, and is not refuted by measurement of things in the world. Thus Hilbert, for example, tried to make a strict formalization of geometry, in which there was no intuitive meaning applicable to any of the terms, such as point, straight line, or coincidence.¹⁰ On the other hand, there have been geometers, e.g. Reimann and Helmholtz, who held that the axioms of geometry "were results of physical observation, and that, therefore, the theorems were of no greater certainty than any statements of physics."¹¹

⁹Cf. Chapter III of Philosophy of Science.

¹⁰Cf. Foundations of Geometry, 1899.

¹¹Frank, Philosophy of Science, 84.

There have also been attempts to coordinate the physical and mathematical realms of geometry. Here the work of Poincare, Einstein and Bridgman has been most influential. "According to Poincare," Frank states,

... the terms which are defined by a system like Hilbert's are physical things. ... If we want to check whether a triangle of light rays in empty space actually has an angle-sum of two right angles, we face a particular difficulty. If we find that the sum in question is different from two right angles, we can also interpret the result by saying that the 'defect' is not due to the non-validity of Euclidean geometry, but to the fact that the rays have been deflected by some hitherto unsuspected law of physics. ... Poincare concluded that we can check whether or not light rays fulfill the Euclidean axioms only if we know all the physical laws about light rays.¹²

Einstein felt that one could, in a practical sense, test the validity of Euclidean geometry. He says, in a paper "Geometry and Experience," in 1921,

According to axiomatic geometry, only the logical-formal is the object of mathematics; but not the intuitive content that is connected with the logical-formal. ... The statements about physical objects are obtained by coordinating with the empty concepts of axiomatic geometry observable objects of physical reality. In particular: solid bodies behave according to the theorems of three-dimensional Euclidean geometry.¹³

One way of relating physical and geometrical axioms was developed by Percy Bridgman, whose ideas of 'operational definition'

¹²Ibid., 86.

¹³Cited in Philosophy of Science, 87.

have been one of the primary break-throughs in modern science. Bridgman says, for example, "The concept of length is, therefore, fixed when the operations by which length is measured are fixed. That is, the concept of length involves as much as and nothing more than the set of operations by which length is determined."¹⁴ It must be noted that the conventional nature of geometry does not mean it is just whimsically arbitrary. If there were no bodies in the world which obeyed Euclidean geometry, for example, it would be only a logical exercise. One recalls that geometry was first developed by Egyptians and Greeks trying to solve practical problems about the world they observed.

Recall that Einstein's ideas showed a close connection between distance and time. Thus, one can talk about a point event alternately in space at time t_1 and another point event at time t_2 . One can talk about an event as having four coordinates, three of which are spatial, and one of which is temporal. To unify this process, and make it simpler, Minkowski suggested in 1908 that one can construct a four-dimensional space to describe events. Thinking in this way, one could describe motion as a static curve in the four-dimensional space. In this system, each event might have one set of four coordinates relative to one reference system (S), say x , y , z and t ; while relative to another reference system (S') it would have another set of coordinates, x' , y' , z' and t' .

¹⁴Logic of Physics, 1927, cited in Philosophy of Science, 87.

There is no way to determine which reference system is truer, and by this argument, there is no way of saying one time (e.g. t as opposed to t') is absolute. For this reason, some persons have suggested that the world is 'really' four-dimensional, and that all events in time are really already established. The apparent progression of events is just due to one's taking up different 'positions' in the time (t equals a constant) plane. Yet Frank points out that this 'deterministic' view is really misleading, for in actuality, men deal only in the 'now' plane (where t equals t_0). As Frank says, "By 'now' we mean the cross section of the four-dimensional space-time continuum that is defined by $t=t_0$. Therefore, it is self-contradictory that any future instant of time $t=t_0$ can exist 'now'."¹⁵ The four-dimensional way of speaking is a useful tool, but it does not tell us about 'reality'.

6. Relativity or Relativism?

It has often been suggested, by some favorably, and by others unfavorably, that relativity supports the belief that there is no permanent truth - nothing upon which men can count, as the basis for their human behavior. Truth, it is said, has become completely subjective, under the influence of modern science. It is only a matter of viewpoint - of the position of the observer, as some have said.

Frank has addressed himself to this criticism in much of

¹⁵Philosophy of Science, 162.

his writing; increasingly so in his later work. In general, his answer has always been of this form: Scientists, as scientists, are never concerned with the underlying reality of the world. What they undertake to do is develop theories which can lead logically to the events that are observed. They are never certain that any theory is true in any absolute way. This is not to say that there is no truth. It is to say that the scientist wants to have practical consequences from his ideas, but not metaphysical interpretations. Thus, for example, Frank says,

The physicist in his own scientific activity has never employed any other concept of truth than that of pragmatism. The 'agreement of thoughts with their object,' as the school philosophy requires, cannot be established by any concrete experiment. In practice we encounter only experiences, never an object; hence nothing can be compared with an object. Actually, the physicist compares only experiences with other experiences. He tests the truth of a theory through what one is accustomed to call 'agreements.'¹⁶

In earlier work, Frank puts the idea this way:

If the statement 'length is relative' is understood in its operational meaning, it is a statement about the fact that certain procedures of measurement yield different results, whereas it had formerly been believed that they yield identical results. But if we transplant the statement 'length is relative' without its operational meaning into psychology or sociology or medicine, the word 'relative' is interpreted, of course, in the way in which this word has been traditionally used in these fields of knowledge. It has meant there that all knowledge is subjective or

¹⁶Modern Science and Its Philosophy, 102.

historically and ethnically conditioned. ...

What we can learn from these examples is simply the fact that a presentation of physics is fit to be a part of the unified sciences only if the operational meaning of every statement is explicitly formulated and carefully carried along when the statement is applied to other sciences.¹⁷

This is not to say that there should be a strict dichotomy between facts and values, or experience of subjectivity, and belief in certainty. Such an ostrich view can have no practical value. Further, it is historically short-sighted, since it "ignores the close ties which have always and everywhere existed between man's picture of the physical universe and his picture of an ideal human society."¹⁸ Always Frank tries to differentiate between these two realms without trying to destroy either. In another address, he says, "The work of the scientist is probably not fundamentally different from the work of the poet. 'Reality in its fullness' can be grasped neither by the scientist nor by the poet. Reality can only be experienced, never represented; ... Every presentation, scientific or poetic, proceeds from creating symbols."¹⁹

Science is not an effort to discover reality, so much as it is an effort to describe man's experience of it. Cassirer, following Kant, suggested that reality is somehow determined by

¹⁷Foundations of Physics, loc. cit., 500.

¹⁸"The Present Role of Science," loc. cit., 21.

¹⁹"Contemporary Science and the Contemporary World View," loc. cit., 65.

the laws developed toward such a description. "There is 'objectivity' or 'objective reality'," he says, "because and in so far as, there are laws - not conversely."²⁰ This view like Kantian idealism, places lawfulness in the processes of human reasoning, rather than in an outside world. In Frank's opinion, Cassirer represents an effort to work a middle ground between science and traditional philosophy. He does not, in spite of his idealistic language, feel that modern physics is a confirmation of Kantian idealism. Freedom, for example, is not proven by the discovery of areas of uncertainty in nature, such as the incompatibility of exact momentum and exact position of particles. Cassirer says in this regard, "In itself it would be very bad for ethics and its dignity if it could not maintain authority except by watching for gaps in the scientific elucidation of nature and, so to speak, creeping into these gaps."²¹

Einstein's own views about the relation between theory and object are enlightening. He apparently did not think that his relativity theory eliminated the possibility of transcendent laws. In an argument concerning chance happenings, he once said, "The Lord God does not throw dice." Einstein upheld the existence of laws in nature as a basic tenet of his faith. He said, for example, "The most incomprehensible thing about the world is that

²⁰Cited in Modern Science and Its Philosophy, 178.

²¹Determinism and Indeterminism in Modern Physics, 1937, cited in Modern Science and Its Philosophy, 183.

it is comprehensible."²²

Similarly, to the Conference on Science, Philosophy and Religion in 1940, Einstein tried to show a connection between faith and the idea of cosmic order. "To this there also belongs the faith in the possibility that the regulations valid for the world of existence are rational, that is comprehensible to reason. I cannot conceive of a genuine scientist without that profound faith. The situation may be expressed by an image: science without religion is lame, religion without science is blind."²³

7. Divergent Views of Relativity

Frank points out, against the metaphysical misinterpretations of Relativity, that almost every conceivable ideology has used Einstein's ideas to support it, although the conclusions reached by each of the various ideologues have been in many cases altogether incompatible with those of the others. There are as many people who decry modern science for supporting materialism as there are those who uphold it for returning an element of idealism and Divine purpose to the world. The Soviets, for example, starting with Lenin, have lambasted relativity, on two points which, in their estimation, are antimaterialistic. Their first objection is that the abandonment of ether as a material medium for light propagation is inconsistent with sense-perception and logic. Their

²²"On Physical Reality," 1936, cited in "Einstein, Mach and Logical Positivism," loc. cit., 284.

²³In "Einstein, Mach and Logical Positivism," 285.

second objection aims at the statements which try to show that one cannot say the earth is 'really' moving, or that the Ptolemaic system is 'really' wrong.

If the Soviets have criticized Einstein's theories as being inimical to materialism, a raft of writers have welcomed them for bringing the immaterial back into a state of respectability, against the dogmatic mechanism of traditional science. Frank cites Lincoln Barnett, who has helped to popularize Relativity, to illustrate the latter view:

Physicists have been forced to abandon the ordinary world of our experience, the world of sense perception. ... Even space and time are forms of intuition which can no more be divorced from consciousness than our concepts of color, shape, or size. Space has no objective reality except as an order or arrangement of the objects we perceive in it, and time has no independent existence apart from the order of events by which we measure it.²⁴

Similar ideas are suggested, and cited by Frank, in writings of British Philosopher H. W. Carr, French Philosopher Henri Bergson, Sociologist P. Sorokin from Harvard, Yale Biologist E. W. Sinnott, the British Astronomers J. Jeans and A. Eddington, and, to a lesser extent, by A. N. Whitehead.

On the other side, there have been suggestions that relativity theory supports materialism. For example, the Nazis, who were very conscious of gaining scientific support for their ideas, opposed Einstein because he was a Jew and a materialist. Simi-

²⁴The Universe of Dr. Einstein, 1948, cited in Philosophy of Science, 176.

larly, some aspects of Einstein's theory have been upheld by Soviet writers for their effect against the static metaphysics of Newton, against which dialectical materialism struggles.

In all these conflicting ideas, Frank points out, there is an element that is common. The misinterpretations all stem from the false application of scientific statements by ignoring their context and their operational definitions. Using the common sense meaning of terms like 'relativity', 'constant', 'conservation' and 'simultaneity' can only lead to confusion. For example, some persons argue that relativity has shown that the universe is really four-dimensional; yet the use of "is" in this sentence is widely different from the "is" in the sentence, "The universe is three-dimensional." The first "is" involves time in a way that is foreign to common usage. Four-dimensionality is only a tool - not a description of reality.

Other persons argue that the convertibility of mass to radiant energy militates against materialism, yet the matter and the energy can both be described by the current theories which employ operational definitions that involve earthly manipulation - just as much for the one as for the other. Again those who hold relativity to be more 'mental' than traditional mechanics should bear in mind the criticisms which were brought against the latter when they were introduced. As Frank points out, all scientific theories are in effect 'mental'.²⁵

²⁵Cf. Modern Science and Its Philosophy, 156.

Finally, the 'subjective' element in Relativity bears comment. Frank states,

The appearance of subjectivity has only been introduced in attempts to formulate the propositions of relativity theory by some analogy to common-sense statements. Instead of saying 'length with respect to a system of reference,' which is not an expression from our common-sense language, we have used the expression 'length for an observer in the system (S).' Then we could say that the table has a 'different length for different observers.' This expression 'for an observer' is formed by analogy to the way in which we express in common-sense language the fact that an object may look different to different observers for reasons of different perspective, or optical illusion or weakness of the eyes. ... However, these analogies become harmful if we forget that they are analogies and regard them as strictly scientific statements. ... The 'observer' will, for example, disappear completely or be replaced by a yardstick or a clock.²⁶

In closing this Chapter, one should remark that in 1950 Frank wrote a book entitled Relativity - A Richer Truth. This work will bear a look more closely in Chapter VII, as it is primarily an effort to vindicate modern science in the face of criticisms that deal with its alleged degrading effect on society. It is well to point out here, however, that the title itself shows there are some metaphysical overtones to relativity which Frank thinks are legitimate.

It is Frank's contention in the book mentioned that relativism is both the most characteristic attitude of modern science and a most useful quality to cultivate. This may at first appear

²⁶Philosophy of Science, 185. Cf. also Encyclopedia of Unified Science, 480.

to contradict the idea, so often repeated throughout Frank's works, that metaphysical interpretations of scientific theories are in fact misinterpretations. However, the question disappears when one sees that Frank is not here using relativity to support a metaphysical view, but rather is starting from a metaphysical principle - in this case, what he calls 'relativism' among scientists - and developing its useful connection to political and social attitudes. His express purpose in this book is to 'interpret the spirit of modern science', and specifically to make an attack on the 'enemies of relativism'. It is Frank's feeling that relativism "has not the slightest thing to do with agnosticism or skepticism, that it is in no way hostile to the belief in ethical or democratic values, that it is accompanying every advance in science and is nothing but a significant representation of human expression which is inseparably connected with our gradually increasing experience."²⁷

²⁷Relativity: A Richer Truth, Introduction.

CHAPTER VI

SCIENTIFIC METHODOLOGY

1. Introduction

In this Chapter we will present Frank's ideas on what might be called the philosophy of science proper - the methods and reasoning processes that are involved in the 'science of science'. We shall examine, as well, what seems to comprise a scientific attitude toward knowledge, including the conception of causality as the basis for scientific prediction, as it has developed during the past century. Frank summarized his conviction about the need for this overview of science, in the article he contributed to the Encyclopedia of Unified Science. There, we read,

Physics has been for centuries the spearhead of advance in human thought. In a unified science it should keep its role as a description of the physical universe and should not deteriorate into an incoherent, and somehow mysterious, agglomeration of symbols, rules, and recipes. In this situation quite a few physicists have tried to avoid all these difficulties by 'sticking strictly to the facts' and by keeping away from the dangerous enterprise of logical and critical analysis. There is no doubt that this attempt is doomed to failure. Scientists who looked at the world from such different angles as Ernst Mach and A. N. Whitehead have agreed on one point: a physicist who dodges all logical analysis and tries to be a 'physicist and only a physicist' will imbue the presentation of his subject with some 'chance philosophy,' usually a very obsolete one.¹

¹Introduction to Foundations of Physics, loc. cit., 428.

Besides this 'involvement' in critical analysis which Frank considered to be part of a worthy scientist's role, Frank came later to include an even broader perspective. Ernst Nagel has described Frank's view of the philosophy of science as a "catholic one." Nagel says,

Thus, on his view, the logical analysis of the language of science, or the articulation of precise rules for assessing evidence in conducting scientific inquiry, are undoubtedly centrally important tasks that fall into its province. But as he saw it, a philosophy of science that is limited to the discussion of such matters is incomplete; and to be adequate, it must also include certain socio-historical considerations. ...²

2. The Need for Historical Perspective

Over and over again, Frank talks of the need to look at the history of science in order to understand the sources for the many misinterpretations it supports. The criticism that modern science is mental, and unnecessarily abstruse, for example, is often bolstered by comparing, say, Einstein's idea of relativity with Newton's laws of motion, which latter are held to be intuitively evident. Yet a cursory glance at history would reveal that Newton himself was criticized for being abstruse and mathematical, at a time when the organismic views of Aristotle were believed to be intuitively evident. This confusion stems from the fact, often pointed out in Frank's works, that a scientific system will eventually be incorporated into daily thinking, and as it were, will

²"Philipp Frank and the Philosophy of Science," in Philipp Frank, 1884-1966, loc. cit.

become petrified into a metaphysical view of life. Frank says, for example, "Newton's scientific theory became a 'philosophical system'."³ Again, elsewhere, he says,

Aristotle's philosophy of physics is a petrification of a physical theory which covered the experiences of Greek and oriental artisans about physical phenomena. Kant's Metaphysical Principles of Natural Science is a petrification of Einstein's relativity and Bohr's quantum theory.⁴

If nothing else were to be accomplished by the history of science, as a study, it would at least tend to instill in scientists and those who look at science in relation to the whole culture, an attitude of modesty. Such an attitude has been described by Heisenberg as characteristic of modern science.⁵ The cause for such modesty is the recognition of so many major reversals in scientific world theories, and the realization that each theory had been the foundation of a large and unyielding superstructure of philosophical views about the world, as it 'really is'.

Nowadays, there may at least be less tendency, among first-rate scientists, to go so far. This modesty is evident in the behavioral sciences, which have reacted, for a time, against the theorizing of Freud. Of course Freud was not just an interpreter

³Philosophy of Science, 35.

⁴Foundations of Physics, 428.

⁵Cf. Frank's "Man Confronts Himself," a review of Heisenberg's The Physicist's Conception of Nature.

of science; he was a first-rate scientist himself. But many have argued that his generalizations were too broad, and too far removed from observation to be validated. After a period of strict, almost picayune, experimentation and observation, there has been a gradual resurgence of theory-making among psychologists.

The need for historical perspective is also seen in the desire to understand older ideas more explicitly and thoroughly. One is able to see more clearly the correct application of an outmoded theory after it has been replaced, threatened or modified by a newer one. In mathematics, for example, Euclidean geometry was so well entrenched before the nineteenth century that one could not see its limitations. With the rise of non-Euclidean axioms, as, for instance, in Riemann's work, the former system came into sharper focus, and therefore its application could be better defined, with practical benefits.

The Copernican revolution is an older example of the same broadening of perspective. Of it, Frank says,

To understand a phenomenon means to interpret our present experience as the repetition of a similar phenomenon of the past. This is true in science, but it is true as well in history. Today ... we understand the Copernican revolution better than nineteenth-century scientists did because we are contemporaries of the Einsteinian revolution.⁶

Frank suggests that a proper view of the Copernican revolu-

⁶Modern Science and Its Philosophy, 216.

tion can give us a key to 'the central problem of all philosophy of science,' that is, to relate the world of sense observations to that of general principles. It can help us also, he adds, "to appreciate the immense gap between these two worlds."⁷

3. Causality: Absolute or Pragmatic?

In spite of Hume's criticism, it has been felt in traditional philosophy and assumed by scientists generally that causality is somehow basic to all scientific thought, if not to all reasoning. The discovery of laws, and their use in predicting events, presupposes the concept of causality. It is evident, however, that in spite of its ancient and wide-ranging roots in western thought, since the Greeks, and its almost universal use in everyday language, the idea of causality is a difficult one to pin down. Frank has had much to say about causality, in his earliest writings, and down to his last. The first major paper he published, after his doctoral work was completed, was "Kausalgesetz und Erfahrung" in 1907. This paper appears as Chapter I of Modern Science and Its Philosophy.

Frank remarks that the paper was kindly received by Einstein, although the latter criticized some of its overstatements. Frank's purpose in the paper was to present causality as another of the principles along with inertia, and the conservation of energy, which are neither empirical nor a priori, but rather are

⁷Ibid., 217.

"purely conventional definitions depending on human arbitrariness."⁸

The argument runs like this. The most general formulation of causality is the following: If a state A of the universe is once followed by a state B, then whenever A occurs B will follow it.

It is important to understand (Frank notes) that the law can be applied only to the whole universe and not to a part of it. This, however, makes it impossible to test the law empirically. In the first place, one can never know the state of the whole universe, and in the second place, it is in general not certain whether it is possible for a state A of the Universe ever to return.⁹

How does one know that state A has in fact returned, even in a limited region of the universe? One might say that if all the perceptible properties were the same, the state would be the same. But what of the imperceptible properties, such as the internal structure of a magnetized piece of iron, which might outwardly appear the same as a non-magnetic one? One would have to include these, too, even if he did not know what all of the imperceptible properties were. In this case, one could always justify causality by saying that, in cases where it appeared to fail, he had not accounted for some relevant property. Thus, Frank concluded, "The principles of pure science, of which the foremost is the law of causality, are certain because they are only disguised definition."¹⁰ However, Frank concludes, this argument holds only

⁸Ibid., 53.

⁹Ibid., 54.

¹⁰Ibid., 57.

for science. "With the question of world conception in the ethical-religious sense, all this has nothing whatsoever to do."

In some later writings, Frank modified this early presentation somewhat, or rather, one should say he took a different view of what should be called the law of causality. For example, in one chapter of Modern Science and Its Philosophy, written in 1930, he discusses the relation of school philosophy to twentieth-century physics, allowing for a statistical interpretation of causality. He says, "If the symbols conform to the experiences in a very detailed manner we speak of causal laws; if the correspondence is of a broader sort we call the laws statistical."¹¹

An example of statistical causality is seen in the effort to predict the behavior of a single electron in a beam. One can state the general distribution of electrons in cases where a great many of them are concerned, but cannot predict the single event. Frank points out, too, that even in so-called single events, such as the striking of a target with a large projectile, one is limited as to accuracy of prediction, generally talking about the 'mean' behavior of a number of such projectiles. He closes this discussion with the observation that it is not necessary, "beside the thriving tree of science to assume a sterile region in which reside the eternally insoluble problems." Instead the enlightened scientist will gradually clarify such problems as space, time, and

¹¹Ibid., 119.

causality in the same way that he perfects his observations. There are no boundaries between science and philosophy, Frank concludes, if one sees science the way Mach suggested one should - 'to order the perceptions systematically and from present perceptions to draw conclusions about perceptions to be expected.'

In 1938, Frank viewed Cassirer's thoughts about causality to be akin to his own, and therefore praised the latter for bringing about a "disintegration" of the classical metaphysical views of this law.¹² In the same year, Frank restated his thoughts about causality in the Encyclopedia of Unified Science, in much the same way as in the 1930 article. In the Encyclopedia he suggested, under the section heading "Physical Reality and Causality," that "One must not exaggerate the gap between the new mechanics of small masses and the Newtonian mechanics." To say that causality holds more strictly in the latter would be "a very inadequate description of the new mechanics," because the two systems cannot be compared fairly. They use different state variables to describe physical reality. In Newtonian mechanics, one speaks of position and momentum; in quantum theory, one cannot give an operational definition for both of these in the same description.

Frank re-examined causality twenty years later, in Chapters

¹²Cf. "Bemerkungen zu Ernst Cassirer: Determinismus und Indeterminismus in der modernen Physik," which is translated in Chapter IX of Modern Science and Its Philosophy.

XI and XII of his Philosophy of Science, which are entitled, respectively, "Causal Laws", and "The Principle of Causality." His ideas do not differ fundamentally in the two places. According to Frank the idea of causality stems from the Judaeo-Christian belief in an Omniscient Intelligence in whose mind the future is "pre-determined" by all past and present events. But for science, "What matters is not that the future is determined, but how it is determined."¹³

One of the primary difficulties of the principle of causality is that one cannot find, with any certainty, which variables in an initial state (A) are relevant, so as to know when this state (A) returns again. This is even more true in field physics than it is in Newtonian mechanics, since in the former, variables are continuous functions, rather than discrete. "The greater the number of state variables," Frank points out, "the smaller is the factual content of the principle of causality."¹⁴

One can never know if he has left out some important variable. If the number of variables becomes infinite, as the original view of causality might suggest, then the principle becomes tautologous. For practical purposes, of course, one assumes a small number of state variables, e.g. a few kinds of force like gravity

¹³Philosophy of Science, 262.

¹⁴Ibid., 274.

and electromagnetism. "We have a choice," Frank concludes, "between making the principle of causality precise and tautological or vague and factual."¹⁵ The tautological form can be formulated simply as follows: If A recurs then B recurs also, .. is the principle of causality. But if A never recurs, then the principle is valid whatever happens.

It is obvious, according to Frank, that "the world process has happened only once. If we regard causality as the recurrence of sequences, it makes no difference whether we say that the world process as a whole obeys or disobeys the principle of causality."¹⁶ For this reason, it is practical, and true of science, to consider only 'subcycles' and 'approximate' recurrences.

There is another point that one must consider, in examining this subject. It has to do with the way in which one determines or not that the original state (A) has recurred. In the example mentioned above about the outward appearances of magnetized and non-magnetized pieces of iron, it was assumed that one could, by tests, establish whether or not the iron was magnetized. As Frank says, "Thus we can learn that if a certain group of experiments performed with a piece of iron yields certain specific effects, the iron will also have the effect of a magnet."¹⁷

¹⁵Ibid.

¹⁶Ibid., 285.

¹⁷Ibid., 187.

Yet here one sees that he has inadvertently assumed the principle of causality with respect to his tests. This is, in effect, the argument which Kant put forth, while trying to refute the disastrous criticisms Hume leveled at the notion of cause. In other words, as Frank says, "We must take the validity of the causality principle for granted if we wish to be entitled to say that a certain experiment reveals to us a certain property of a body; we must assume that the result of our experiment is the 'effect' of this property."¹⁸

Frank makes a final point that is important. Laws, as such, are distinct from the phenomena they purport to explain. The connections between the laws and the observables are never perfect. Thus, although the mathematical statement of a law may be exact, its confirmation can only be approximate, and needs many repetitions to see the limit which it approaches. In Frank's words, "If we speak in terms of observable phenomena, all laws are statistical."¹⁹

Certain statistical laws, like the firing of a projectile, admit of limits. These Frank calls 'causal laws.' But there are also statistical laws which approach no such limit. The tossing of a coin is an example. Although one can formulate the statistical law that the frequency of heads and tails will tend to be

¹⁸Ibid.

¹⁹Ibid.

equal as the number of tries increases, he can never say what the physical behavior of the coin will be on any specific toss.

To sum up, Frank holds that the law of causality can have no meaningful place in science unless it is couched in terms that can be operationally defined. Even in such cases, it is a vague law, and does not apply equally to all phenomena.

4. Induction and Logic in Scientific Method

What is "The Scientific Method"? Sometimes it is suggested that science differs from non-science by proceeding from particulars to generals, in a process called induction. This is in contradistinction to the derivation of particulars from general principles - the method of mathematics and theology called deduction. However, Frank has shown that such a distinction is a one-sided picture. Both inductive and deductive phases belong in science - perhaps simultaneously, although they are analyzed separately for the sake of description.

Briefly put, the scientific method is a full circle, which may be described as follows: The scientist observes nature; then he suggests a theory which may explain what he sees; from this theory he deduces hypotheses which are testable; finally, he experiments to validate or refute the hypotheses. If the tests bear out the hypotheses, he says the theory is confirmed; if not, he repeats the cycle again, making modifications along the line, where necessary.

The deductive phases of the cycle present the least prob-

lems. They follow the rules of syllogistic reasoning that have been known from at least as far back as Socrates. It is the inductive phases which are the most intriguing, because they do not easily lend themselves to rules, or even to exact description. For this reason, the philosophy of science can be called the effort to discover the meaning of induction.

When it comes right down to cases of scientific theorizing, one is hard put to describe just what the scientist does to derive his theory. There seems to be a certain openness involved - a gap between the facts and the theoretical description - which the scientist must bridge in his mind; it is a gap that allows of intuition and artistry, more than exact method. Frank notes this in many places, and cites, for example, Einstein and Carnap as two famous scientists who are in accord with the idea.

J. H. Woodger, on the other hand, seems to fault Frank for taking an unnecessarily informal view of scientific method. In a review of Frank's Modern Science and Its Philosophy, Woodger discusses Frank's views on what, exactly, physicists observe, and how the things they observe are related to the statements they make in their notes. He seems to suggest that a proper logical analysis of scientific methodology would in fact lead to a precise formula which one might go about in order to find scientific truth. Woodger says,

The problems will never be properly cleared up
until someone carries out the laborious task of making

a complete formalization and axiomatisation of a small part of physical theory, showing in minute and convincing detail every step of the procedure from the laboratory bench to the fully elaborated theory. The Polish methodologists have shown us how this is to be done. But one curious feature of this book is the complete absence of any mention of the great Polish school of methodology which flourished between the two world wars and contributed so much to the clarification of the problems discussed in these essays and greatly influenced some members of the Vienna Circle. The names of Kotarbinski, Lesniewski, Lukasiewica and Tarski do not appear in its pages.²⁰

This criticism is typical of the comments which Frank's works receive, faulting him for an apparent oversight, when in reality, Frank simply does not agree with what the critic says. The article is also typical in misconstruing Frank's informal style and lack of minutia for a shallowness of knowledge of his field, which Woodger, and other critics, are eager to supplement. The truth is, as has been pointed out above, Frank's knowledge of these subjects is encyclopedic, and his broadly general presentations represent a distillation of a thorough scholarship - the "cream", as one of Frank's students called it.

With regard to the place of the scientist's personality in bridging the gap between facts and theories, Frank cites Ernst Mach, who described induction in these words: "Above all, it is not a logical process although such processes can be inserted as intermediary and auxiliary links. The principal effort that

²⁰Cf. British Journal for the Philosophy of Science 2, 1951, 168-169.

leads to the discovery of new knowledge is due to abstraction and imagination.²¹

As noted earlier, it is clear that the scientist does not simply 'read off' his theory from the facts he observes. A theory which is a simple enumeration of observations is not a theory at all; it is a catalogue. Such a listing could have no use, because it would be just as complicated as the world it 'described.'²² Instead of this approach, there must be, on the part of the scientist, an intelligent guess as to a simple and exact symbolic representation of things which are neither simple nor exactly knowable. In this sort of enterprise, the greatest scientists are necessarily also the greatest simplifiers of nature. That is, they are those who can symbolize an infinitely various nature, of which they have observed a small piece, in language that can be grasped by a finite mind.

Newton was able to synthesize into one system the realms of moving bodies, the celestial gyrations, and the changing tides. All of these had been carefully described for hundreds of years before him; it was his special contribution to see their interconnectedness. Such an ability indicates a strong conviction on Newton's part that there was indeed an order to be discovered. It is the same conviction which one finds in Einstein's words, cited

²¹Philosophy of Science, 319.

²²One wonders, in light of this, what use is to be performed by various governmental agencies which gather 'statistics', on subjects which are not yet under study, on the assumption that they may soon be.

by Frank, "The most incomprehensible thing about the world is that it is comprehensible."²³

Such an overview, or synthesizing approach, is the area in which philosophy and science meet appropriately. It belongs to what Frank calls metaphysics. Philosophy in this sense deals with more than the search for goals to guide scientific knowledge. It also influences science proper by entering into the psychological make-up of the scientist, and coloring his choice of theories, coloring the experimental tests he deduces from the theories, and even coloring the very observations he makes.

Frank has addressed himself to the subjective element in science, in many places. In a paper of 1954, for example, entitled "The Variety of Reasons for the Acceptance of Scientific Theories,"²⁴ he speaks of some scientists having a 'double standard' of truth, including the 'hard facts' of their work, and the metaphysical or religious beliefs they carry outside their work. This duplicity is bound to arise if one considers science to be a 'picture of reality', or a 'collection of facts'. Actually, no theory is in full agreement with facts, let alone a complete picture. Instead, as Frank says, "several theories that are in partial agreement [present themselves], and we have to determine the

²³Cf. "Einstein, Mach and Logical Positivism," loc. cit.

²⁴Appearing in The Validation of Scientific Theory, ed. Frank, 1956.

final theory by a compromise."²⁵ This compromise always takes into account the use to which the theory will be put. In Frank's words, "Science actually is an instrument that serves the purpose of connecting present events with future events and deliberately utilizes this knowledge to shape future physical events as they are desired."

5. Validation of Theories

Frank discusses in several places the changes that have occurred historically in the reasons for accepting scientific theories. The ancients are said to have proceeded primarily by deduction. For example, it is said that Aristotle started with principles of philosophical generality, such as the idea of 'natural place' to which bodies like earth or fire, tend. However, practical science has always used both induction and deduction. As Frank points out, "Aristotle did not find his general principles in his dreams, but advanced them on the basis of experience that consisted in the sum of the individual facts that had been observed."²⁶ He continues, "The difference between ancient and modern science was not the use of induction - ancient science was based on induction as is modern science - but the criteria by which a discovered principle was recognized to be valid."

²⁵Op. cit., 13.

²⁶Philosophy of Science, 298.

"How are theories validated?" "Why are they accepted?"

These questions have two different answers, and one should consider them separately. With respect to validation, one might at first think that any theory which can lead by logical steps to statements confirmable by experiment could be called valid. This is not so, as Frank points out, with a bizarre example from Russel.

We start from the assumptions that 'bread is made of stone,' and that 'stone is nourishing.' Then it follows logically that 'bread is nourishing.' This statement can be confirmed by experiments. If someone claims for this reason that we have confirmed our assumptions, he would certainly be ridiculous. According to Russel, a great many confirmations of physical hypotheses are of this type.²⁷

It has sometimes been felt that the calculus of probabilities, a branch of mathematics, can be employed to establish the validity of physical theories. The process would be to establish the degree to which the theory is borne out by experimentation that aims to test it. Frank presents at some length different theories of probability which have been argued in the last generation, including especially the ideas of Reichenbach, Carnap, and Bronowski.

Reichenbach, and Richard von Mises, for instance, have been strict adherents to the "relative frequency" view, which can be explained as follows: Reichenbach proposes two methods of computing probability. In the "probability of the first kind," one may

²⁷Foundations of Physics, 431.

regard as the basic collective all facts which can be logically derived from the theory. If there were 'n' of these facts, and 'm' of them were actually observed in experiments, the probability of the theory would be given by 'm/n'. In the "probability of the second kind," the basic collective is the realm of observables which have been explained by a group of theories. Let us say that there are 'n' observables, e.g. the behavior patterns of light in a certain optical system; and the theories proposed are the Huyghens Wave theory, and Newtonian mechanics. If the first theory were to account for 'm' of these observables, and the second to account for 'k' of them, the probability of the former would be 'm/n', while that of the latter would be 'k/n'. Actually, these two kinds of probability differ only in the operational definitions which describe their application.

There are problems in trying how to decide which application of the calculus of probabilities is appropriate. For example, one might suggest that if he threw a switch to turn on a light one thousand times, and the light went on in all but the last trial, then the probability of the light's going on the next time would be 999/1000. Obviously, on the contrary, one would expect the light to fail the next time.

Hilda Geiringer suggests another example against the 'relative frequency' model. Writing in a paper of 1938, she says,

Let us assume that someone advance the hypothesis H that 'every triangle has an obtuse angle.' In order

to test his assertion, we picked out a hundred triangles at random and measured them. The result may be that H is right in seventy cases and wrong in thirty cases. Then the scientist would obviously say that "H is wrong," and not that it is 'valid with a probability of 70%.'²⁸

Carnap presented yet another view of probability, which has been called 'logical probability' and 'inductive probability.' It is to be used in cases where the relative frequency concept cannot be applied. It is logical rather than empirical, because it considers not the truth of actual observations, but the logical ties between these observations as evidence (e), and the hypothesis (h) which purports to derive them. Thus, for example, one might examine the realm of Kepler's hypothesis about the elliptical orbits of planets. By Carnap's analysis, one should expect that any planet would lie between two ellipses whose separation depends upon the range of error one might foresee. The area between these two ellipses Carnap would call the measure of the hypothesis $[m(h)]$. Each observation of a planet would yield its position within a certain range of error, which would constitute a small circle on the star map. If one added up all such little circular areas that correspond to the observed positions of the planet in question, he would have a measure of the evidence $[m(e)]$. The areas which are held in common by the observed and the theoretical positions is $m(esh)$. Carnap's thesis then may be stated thus:

²⁸"On the Probability of Hypotheses," cited in Philosophy of Science, 326.

The inductive probability of Kepler's hypothesis is $\frac{m(e \& h)}{m(e)}$.

There are many cases, as Carnap himself was quick to point out, which cannot be described so simply. "For instance," he says, "we cannot expect to apply inductive logic to Einstein's general theory of relativity."²⁹ However, as Frank is quick to remind us, this lack of universal application does not destroy the usefulness of Carnap's idea; it just makes it another case, like the law of causality, where the applicability depends upon the kind of phenomenon we are trying to describe.

In spite of the differences between Reichenbach's and Carnap's approach to probability, in many cases of actual application, both methods come to the same result, and the arguments raised against the former apply equally to the latter.

Jacob Bronowski, in 1953, brought in another criterion for validity, in order to avoid such problems. It is the idea of simplicity or economy, applied in a new way. In cases where two theories have the same degree of statistical or inductive probability, Bronowski's suggestion is that the simplest theory is the most probable. As an example, one might compare the ideas of Copernicus and Ptolemy. Both theories lead logically to the observables, but Copernicus' theory is more acceptable, because it is more economical.

²⁹Cf. The Validation of Scientific Theory, 14.

6. Acceptance of Theories

There are a variety of reasons which Frank puts forward for why scientific theories are accepted in practice, not all of which are strictly scientific or logical. The desires and convictions of the scientist play their roles too. One might think at first that scientists would accept or reject a theory solely on the basis of its ability to support observable evidence, and that any so-called 'counterfactual case' would be grounds for eliminating a theory. But an honest look at any number of actual theories in practice will reveal that this is not always true.

For example, it has been suggested that the earth was thrown off the surface of the sun while the latter was still in a molten state, in what has been called the "Tidal Crest Theory". Considering the forces of gravity on the earth, and the liquidity of its original state, one would expect that the earth would have assumed a perfectly symmetrical shape, approximately spherical. However, the careful observations accrued during the recent International Geophysical Year (IGY) showed that the earth is in fact plum-shaped. This has not killed the Tidal Crest Theory, but it has caused it to be modified.

One might also, as a mental exercise, imagine that one day it is observed that a body thrown up from the surface of the earth does not come down again, but continues on up out of sight. Such a happening is inconceivable because the law of gravity is so well established by experience. In an event of this sort, one

might undertake any amount of research to discover another reason for the unexpected behavior of the body than the failure of gravitational law. And if, after this research, one should fail to find a suitable explanation, he would give it up as an anomaly, without ever doubting the law of gravity. This, in spite of the general rule of logical analysis that states, 'If p implies q, and not q is the case, then not p is the case,' which is the form of all such counterfactuals.

Even in cases that are less unlikely, scientists are apt to refuse to change their views about a theory, unless arguments other than purely logical ones are proposed. Nor is this attitude necessarily illogical. Frank has pointed out that "we never have one theory that is in full agreement [with observation] but several theories that are in partial agreement, and we have to determine the final theory by a compromise."²⁹

Because theories are always incomplete as well as very complex, one cannot refute them simply by pointing to one area in which they fail to explain the facts. Frank says this, in effect, when he discusses the possibility of a so-called 'crucial experiment'. In Frank's words,

A single experiment can only refute a 'theory' if we mean by 'theory' a system of specific statements with no allowance for modification. But what is actually called a 'theory' in science is never such a system. If

²⁹Cf. The Validation of Scientific Theory, 14.

we speak of the 'ether theory' or the 'corpuscular theory' of light, or of the 'theory of evolution' in biology, each of these names covers a great variety of possible systems. Therefore no crucial experiment can refute any such theories. A famous example was the 'crucial experiment' which Arago proposed in 1850 to test the corpuscular theory of light. This theory was refuted in 1855, but in 1905 Einstein again made use of this theory in a greatly modified form known as the hypothesis of 'light quanta' or 'photons,'³⁰

Two criteria for the acceptance of theories were mentioned above, namely, agreement with facts, and simplicity. At times these two are mutually exclusive, in which case a compromise between them becomes necessary. That which makes a scientist lean toward one theory or another is a complex matter. The concept of simplicity, for example, involves a whole universe of discourse - not just a mathematical demonstration.

Frank suggests at least the following sorts of consideration in the acceptance of a theory: Conformity with religious belief, as, for example, a belief in miracles; conformity with the philosophy of the day, such as Aristotelian view of nature; the dynamic or fertile nature of the theory, as demonstrated by its ability to generate other ideas; the compatibility of the theory with common sense or intuitive feeling, as Newtonian mechanics exhibits for most people today; and lastly, the support of desired moral behavior.

By way of analogy, Frank gives an effective, if somewhat oversimplified, example of such a compromise. In his book Philosophy of Science: The Link Between Science and Philosophy, as well

³⁰Philosophy of Science, 31.

as in an earlier paper, "The variety of reasons for the acceptance of scientific theories,"³¹ Frank likens the problem to "the choice of an airplane." He suggests, for instance, that one can "enjoy the beauty and elegance"³² of the plane, as well as that of the theory. One cannot speak of the truth or the perfection of the theory or the plane, in any absolute sense. Rather, Frank says, "We can only ask whether it is 'good' or 'perfect' for a certain purpose." In the plane's case, such a purpose may be speed, or endurance, or safety, or fun, or 'convenience for reading or sleeping.' "It is impossible," Frank notes, "to construct an airplane which fulfills all these purposes in a maximal way; we must achieve some compromise. In order to determine the kind of compromise which 'should' be achieved, we must decide which is more important. ... The answer to this question can certainly not be derived from any physical or engineering science."

It is interesting to note that this analogy of finding a useful airplane seems to have upset one critic of Frank. C. W. Kegley labeled Frank's compromise between the technological and sociological value of a theory as "incredible." He said, "It underscores the confusion of acceptance by the general public and by scientists, and confuses the grounds of acceptance by scientists."³³ He also called "astonishing", what he interpreted as

³¹In The Validation of Scientific Theory.

³²Philosophy of Science, 356.

³³"Philipp Frank on Scientific Theory and its Relation to Other Inquiries," in Atti del XII Congresso Internazionale di Filosofia, Vol. II. Florence, 1960, 231-237.

Frank's demand that scientists criticize and determine the 'human value' of their discoveries or theories. Finally, in support of his criticism, Kegley quoted Frank out of context, thus: "He (Frank) writes that 'the ultimate foundations of all sciences are strongly influenced by sociology and anthropology.'" Frank's actual idea was as follows: "Philosophers of scientific background who have attempted to consider the human enterprise as a whole have claimed repeatedly that the ultimate foundations of all sciences are strongly influenced by sociology and anthropology. ... I submit that there may be much truth in it."³⁴

Perhaps the farthest Kegley strayed from a valid interpretation of Frank's ideas is when he suggested that the latter looks upon induction as a process of "deriving scientific statements from general principles."³⁵ That this is false has been shown above, in Chapter III, Section 4.

³⁴Frank, "The Present Role of Science," in Atti del XII Congresso Internazionale di Filosofia, Vol. I. 1960, 12.

³⁵Kegley cites this idea from Frank's "Principles of Science," p. 23. Perhaps he means to say "Philosophy of Science," p. 23. There, Frank discusses the chain between science and philosophy, saying, in part, "This longing to find out 'why' is nothing more than the longing to derive scientific statements from general principles that are plausible and intelligible."

The same criticisms which Kegley raises in this article he raised earlier, in an article entitled "Reflections on Philipp Frank's Philosophy of Science," in Philosophy of Science 26, 1959, 35-40. This article is sharply condemned by F. J. Rutherford, in his article, "Frank's Philosophy of Science Revisited," Philosophy of Science 27, 1960, 183-186. Rutherford calls Kegley's article "hysterical."

7. Scientific Language

Scientific language has been mentioned in passing, several times above, with respect to operational definitions. It is appropriate here to discuss it more specifically. How does Frank view the way in which scientists put things verbally, and especially how does he see that scientific language can lead to misunderstandings? It is well to comment first on the language which Frank himself used.

As was noted above, in Chapter I, one of Frank's most frequently-noted characteristics was his concern for clarity. He wrote in terms that were the most economical and simple he could find, without doing injustice to the complex topics he undertook to discuss. For this reason, one finds it all too easy to overlook the depth of Frank's thoughts. It is only after sufficient time for reflection that one begins to appreciate the import of his statements. His style of presentation has been called deceptively informal, and, as one student noted,³⁶ his 'phillistine' listeners were sometimes disappointed, and apt to accuse him of being lazy.

Frank liked to gear discussions to the specific mood of his listeners, using contemporary, even mundane illustrations, rather than preaching from the authoritative position which he obviously held. In cases where his students were unprepared for the intricacies of theoretical mathematics, for instance, he modified his

³⁶Cf. Chapter I, above.

presentations in language which was familiar to them. One student's comment - that Frank liked to start each class with a resume - points out a characteristic that is evident in all his writing also. His major ideas occur over and over again, being couched in easy, comfortable language, developed each time by many examples from his own experience, as well as from the writings of men he had studied.

In these traits, Frank reflects one of the fundamental concerns of twentieth-century philosophy - first among the Vienna Circle followers and the Logical Empiricists, and latterly, within the circle of so-called Linguistic Analysts, especially in England and America. In Frank's opinion, it is in the developments of language that modern philosophy finds its forte. He says, for example, "The new physics does not teach us anything about 'matter' and 'spirit', but much about semantics."³⁷ And, again, "I believe that the advances in philosophy which have been stimulated by twentieth-century physics, like relativity and quantum theory, are not advances in metaphysics, but advances in semantics."³⁸

In an article entitled "Non-scientific Symbols in Science,"³⁹ Frank discusses the way in which scientific language can be abused, on the one hand by those who advance the cause of anti-

³⁷"The Present Role of Science," loc. cit., 8.

³⁸"Contemporary Science and the Contemporary World View," loc. cit., 63.

³⁹In Symbols and Values: An Initial Study, 13th Symposium of the Conference on Science, Philosophy and Religion, Columbia U., 1952, Harper Row, 1954, and Cooper Square, N.Y., 1964, 341-348.

matter, such as J. J. Thomson and Rutherford, and on the other hand by those, like the Marxists, who hold matter to be good. Both extremes come from taking from scientific symbols certain 'overtones' which are not to be found in their operational definitions. Frank suggests that the positivistic approach, as a "neutral" or non-committal view, is most fruitful, and avoids the pitfalls of the extremes. He says, "'Positivism' attempts to clarify the status of non-scientific symbols in science. It does not deny their legitimacy, provided that this status is kept in mind."⁴⁰

The positivistic view of language, which Frank inherited, stems from a belief, first, in the efficacy of science to solve human problems and to discover truth, and, secondly, from the belief that traditional metaphysics loses all its meaning by incorporating a language of universals which cannot be put to the test of experience. The first manifestations of this latter view were put in reactionary and extreme terms, as in Chapter II.

Mach, according to Frank, considered that "physics is nothing but a collection of statements about the connections among sense perceptions, and theories are nothing but economical means of expression for summarizing these connections."⁴¹ In keeping with this view, Mach advocated the elimination of metaphysics. Similarly, Peirce and James, whom Mach met in Europe, leaned away

⁴⁰Ibid., 348.

⁴¹Modern Science and Its Philosophy, 62.

from metaphysics toward the 'cash value' of ideas, as the criterion for their validity.

The pragmatic view of language led to the formulation of what has been called the 'verifiability criterion of meaning,' among the positivists of the Vienna Circle and its associates. Carnap, for example, held that meaningful discourse is either (1) tautologous (analytical), such as in mathematics or logic, (2) contradictory (and hence, false), or (3) empirical. Since metaphysics is none of these, it must be meaningless. "Metaphysicians," Carnap jibed, "are musicians without talent."⁴²

The strict atomistic empiricism of the earlier positivism faded out in the twentieth century, so that the so-called Logical Empiricists of the Twenties no longer felt that meaningful language is limited to 'observational terms', like red, warm, and spots touching. One might, it was allowed, use abstractions, so long as the statements derivable from the abstractions could be checked by experiment. There are concepts, as Frank pointed out, which "cannot be deduced from sensory raw material."⁴³ Again, "According to the conception of logical empiricism the relations between symbols which form the 'top' of any scientific theory cannot be produced by any logical method. Their origin can only be explained psychologically."

⁴²Cf. Frank's "The Pragmatic Element in Carnap's Elimination of Metaphysics," in The Philosophy of Rudolf Carnap, ed. P. A. Schilpp, Chicago, 1963, 159-164.

⁴³"Einstein, Mach and Logical Positivism" loc. cit., 280.

It is the latter view, rather than the strict 'operationalism' of Bridgman, which Frank developed in his later works. Not that Frank criticized Bridgman unfavorably; he agreed with the latter in respect to the application of language of science qua science. But Frank saw that meaning goes beyond the operational definition - the operations one performs to test the idea. Frank saw the non-scientific aspects of science, for which operationalism fails to hold.

Discussing the courses he taught in philosophy of science, Frank illustrated this more moderate view. "I take great pains," he said, "to present an adequate conception of 'straight metaphysics' and, at the other extreme, 'straight positivism' which bluntly says that there is no principle except those which can be confirmed by the agreement of their consequences with experience."⁴⁴ In this latter group, Frank would include Hume, Comte, Stallo, Peirce, Mach, Poincare, James, Dewey, Bridgman, Wittgenstein and Carnap.

⁴⁴Modern Science and Its Philosophy, 256.

CHAPTER VII

SCIENCE AND CULTURE

1. The Divorce of Science from Culture

As has been demonstrated in numerous examples above, Frank had a great interest in science, not only as a theoretical discipline and a source of truth about nature, but also as it fits into the whole human endeavor. Whatever confidence he may have felt in the processes of science as the best way to think, he recognized the need to integrate science into the liberal education and social philosophy of the culture that developed it. Lacking such an integration, he felt, science had been abused, and misunderstood. The efforts of this Chapter will be to consider more closely the role that science can, and in Frank's opinion, ought to play, for the advancement of society, not only as a tool for its material well-being, but more important, as a way of thinking.

There are natural tendencies for scientists to develop an attitude of uncritical isolationism, and for science to divorce itself from other fields. In the first place, society at large often views scientists, especially university types, as cold and reasoning individuals, who steep themselves in abstractions which have little appeal for the common man. Further, science is also viewed at times with suspicion as an agency for the advancement of

materialism, and the engines of political power. This view has been supported from time to time by cases in which politically naive scientists have become dupes of governmental leaders, or social reformers, who use the scientists' reputation and professional skill to support their own schemes for power. Often scientists, particularly those knowledgeable in the field of war machinery, have been targets for defection operations mounted against major world powers, like America and the Soviet Union.

On the other hand, scientists themselves add to the estrangement by viewing the political and philosophical worlds with suspicion or disgust. They are not at home in the realm of unpredictable human actions. Instead of becoming involved in political and social machinations, or philosophical speculation, they prefer to lead a cloistered life in the safety of their laboratories, where all the variables are known, or at least knowable. Physicists may call the attempts to find metaphysical interpretations of science - for example, the theories of relativity - just so much nonsense. But, as Frank comments, "Unfortunately, this 'nonsense' has a powerful effect upon human behavior, and a physicist who is not able to give his students a precise account of the philosophical repercussions of relativity does not fulfill the duties of a physics teacher in a democratic society."¹

In an article entitled "Science Linked to Life,"² Frank

¹Philosophy of Science: The Link Between Science and Philosophy, 175.

²Saturday Review of Literature 33, May 6, 1950, 25-26.

suggests one way in which scientists can help, through publications, to overcome the tendency to divorce science from culture. Science does not normally advance, he says, through the publishing of books, in the way that other fields do. Einstein's Meaning of Relativity is a rare exception. There should, Frank thinks, be more efforts along this line. For example, one might use unorthodox texts in his courses that place equal emphasis on the mathematical, the experimental, and the philosophical views. This would allow for more comparison and choice on the students' part. Also, more 'handbooks' and other reference materials should be made available. Another important area is in the so-called 'popularization' of scientific findings for the consumption of 'laymen'. Frank calls for more 'literary' works to be written by trained scientists, mentioning by way of illustration the book Joseph Henry, His Life and Work. Of more recent publications, we might add The Microbe Hunters, and The Territorial Imperative. It seems, as a matter of fact, that biology has been far more given to literary interpretation than other sciences. Finally, books that combine humanistic and scientific philosophy are needed. As a good example of this, Frank cites Cassirer's book, The Problem of Knowledge.

Today, looking over the scientific publications since 1950, when Frank's criticism was written, one is surprised and encouraged to see that all the types of writing that Frank mentioned, described above, have undergone an increase of publication. This is especially true in the popularizations of scientific developments,

and more recently, in the up-grading of text materials, such as the Biological Science Curriculum Study, and the Physical Science Study Committee series for high school, and beginning college courses. In view of this one wonders if Frank's early criticism was somehow influential. At any rate, the increases in publications indicate that Frank's opinion of the country's needs in this area has become the generally-accepted opinion.

2. Science as a Balance of Mind

The first place to begin efforts at reuniting science and the humanities is, of course, in the science classroom. A scientist who is knowledgeable of his cultural environment should be particularly well-suited to develop in his students a sense of direction - to give them what Hutchins has called wisdom, namely, "the knowledge of principles and causes."³ In his Philosophy of Science: The Link Between Science and Philosophy, Frank describes the potential educational value of science in terms similar to those of Hutchins. There, he says,

Science has to do, on the one hand, with hard stubborn facts, and on the other hand, with general ideas. What science teaches us is the correlation between both. The chief thing university educators should give to students is interest in the possibility of coordinating stubborn facts by means of abstract principles. This is the most fascinating topic of university education. ... We need a full understanding of the principles of physics or biology, and under-

³Cited in Philosophy of Science, xiii.

standing not only of logical argument but also of psychological and sociological laws; briefly, we need to complement the science of physical nature by the science of man.⁴

This is the potential value of science. However, it is a matter of fact that science teachers often fail to meet their potential, in the classroom. They tend to be dull, sometimes even on purpose, from a misguided sense of duty to what they call 'objectivity'. It is safe to say that most students who stay in science do so in spite of their teachers, rather than because of them. These students are so highly motivated to learn about science that they put up with their education, for the sake of the end.

"There is scarcely any doubt," Frank observes, "that the teachers of philosophy, history, or English have a much greater influence upon the intellectual and emotional make-up of the average college student than the teachers of mathematics or chemistry."⁵ When faced with questions about the meaning of science a propos of the 'relativity of truth' or the 'freedom of the will', Frank claims, "The scientist will, as a matter of fact, often be more helpless than an intelligent reader of popular magazines."⁶ Conventional teaching in science has resulted in uncritical thinking. Frank goes on to observe, "The longing for the integration of knowledge is very deeply rooted in the human mind. If it is not

⁴Philosophy of Science, xiv.

⁵Ibid., xv.

⁶Modern Science and Its Philosophy, 230.

cultivated by the science teacher, it will look for other outlets."⁷

Such other outlets may be popular presentations of science, or worse, even anti-scientific ideologies. Thus, the student with a purely technical training is, Frank continues, "extremely gullible when he is faced with pseudophilosophic and pseudoreligious interpretations that fill somehow the gap left by his science courses."⁸ This writer is reminded of a piece of graffiti that made the rounds of the engineering school he attended. It pictured a country hick, with hayseed in his hair, and a sliderule in his pocket, saying, "Two years ago I couldn't even spell 'Injuneeer', and now I are one!"

Frank recommends that courses in the philosophy of science be made obligatory for all students of science, be it pure science or technology. In the chapter of Modern Science and Its Philosophy entitled "Philosophy of Science in the Physics Curriculum," he outlines such a course, as he taught it at Harvard for many years. "My starting point," he says there, "has been the traditional distinction between 'scientific truth' and 'philosophic truth.' Specifically, I have referred to the formulation that the great medieval philosopher, Thomas Aquinas, gave to this distinction."

From this point, Frank gives a historical survey of principles that have been used by scientists, as a basis for their

⁷Ibid.

⁸Ibid., 258.

propositions. His students studied the mechanistic period in its entirety - approximately 1600-1900 - based on Newtonian laws of mechanics. To show that these latter are not 'self-evident' the students examined the earlier period of Aristotelian or 'organismic' science. Having established this historical perspective, Frank took his students through the development of 'logico-empirical analysis', and showed its application to the most relevant parts of physics today, such as geometry, Newtonian mechanics, the corpuscular and relativistic theories of light, and quantum theory. Similar examination was given to causality, determination, chance, energy and mass. Finally, the course examined the philosophical interpretations of science, and what Frank calls "the link of metaphysical creeds with religious and political creeds."⁸

This is the way in which Frank tried, in his own work, to reestablish the link between philosophy and science. Whatever merits his particular approach to the course may be, Frank makes a strong case for this kind of study in the well-rounded curriculum. He generalizes his convictions about the efficacy of such a program, thus:

If science is taught in this way, the emphasis on science and technology will no longer be an obstacle to a liberal education of the student. The deplorable gap between science and the humanities will not arise, let alone widen. On the other hand, the intensive study of science as a living being will give to the student of it a profound understanding of the role of the human mind in human action, which is the very goal

⁸Ibid., 258.

of instruction in the humanities.⁹

Frank's suggestions about science teaching seem almost prophetic. In the last few years there has been a tendency among science textbook writers, and curriculum study committees, to follow some of the suggestions that Frank has offered. Particularly effective are the Biological Science Curriculum Study, and the Physical Science Study Committee, supported by federal and private funds, which have combined the experience and ideas of thousands of teachers throughout the United States, to develop a unified approach to the teaching of science to high school, and latterly, to college students. One of their principal beliefs is that one cannot simply teach facts; indeed, the explosion of knowledge in the sciences has made it a practical impossibility to learn all the facts, even were it desirable to know them. This Frank would agree with entirely.

The effort has now become, of necessity, a search for generalizations and unifying principles. One is reminded of a story Frank tells about Einstein. The latter was once confronted with a remark Thomas Edison had made about the uselessness of college education. When someone told Einstein what Edison had said, he answered, "It is not so very important for a person to learn facts. For that he does not really need a college. He can learn them from

⁹Ibid., 282.

books. The value of an education in a liberal arts college is not the learning of many facts but the training of the mind to think something that cannot be learned from textbooks."¹⁰ This remark also characterizes Frank's attitude.

3. Scientific Specialization

Sometimes it is held that the depth of detail demanded of a scientist in order to be a leader in his field makes it impossible for him to gain a proper perspective on his study. He has to run, just in order to keep from falling behind, it has been said. Therefore, in order to gain any depth of understanding, scientists must limit their area of study drastically. Gone are the days of the so-called 'natural philosopher' who knew something of all the sciences, and tried to integrate them with an overview. Now there are experts in solid-state electronics, laser optics, crystallography, and the like - topics that are totally foreign to the uninitiated. As Frank points out,¹¹ it seems almost like another Tower of Babel, where there has been a 'confusion of tongues', and the progress has stopped.

How does Frank answer this criticism? He holds that there is actually a unifying trend within science that tends to overcome the disintegrating effect of specialization. As more and more is discovered about particular sciences, which at first seemed to be

¹⁰Cited in Einstein: His Life and Times, 185.

¹¹Cf. "Contemporary Science and the Contemporary World View," loc. cit., 60.

disparate, principles are discovered which can be applied to more than one branch of study, thus reuniting them. For example, the apparent boundaries between traditional 'chemistry' and traditional 'physics' have been broken down, so that one can now study physical chemistry, or atomic physics, as intermediate areas. Much the same elimination of boundaries seems to be taking place as well in the area of bio-physics.

Some educators look for integrating principles to come from so-called perennial philosophies, which will not need revision. For Catholics, this might be provided by Thomism; for Communists, by Dialectical Materialism. But Frank warns that there would be a high price to pay for such permanent unifying principles. The only way to keep such philosophies from becoming outmoded would be to eliminate their direct connection to present reality - in other words, to make them tautologous and irrefutable. Frank does not see this as a satisfactory solution. Instead, he offers the 'scientific method' as the only realistic way of integration. Scientists and philosophers need not agree on ultimate general principles, in order to agree on the practical aims of their education and research.¹²

4. Science and Materialism

It was seen above that science has often been called upon to support varying philosophical and moral codes; indeed, one and

¹²Cf. Relativity: A Richer Truth, Part II.

the same scientific theory has at times been used to prove two contradictory ideas. Because the making of generalizations is a creative, human endeavor, which can go far beyond the collecting of facts, it can often be abused in the sense mentioned. The linking of science and philosophy is often as much a matter of personal taste as it is a matter of logic. As R. Oppenheimer once said, commenting on a paper of Frank's, "Everything cannot be connected with everything; everything can be connected with anything."¹³

Does science encourage materialistic thinking? Although one cannot say that a particular scientific theory supports this or that metaphysical system, or moral code, is there not some evidence that scientific thinking in general supports materialism? After all, scientists look for causal laws wholly within the material realm, and do not consider any other realm.

It appears that Frank's view of this question changed somewhat toward the end of his life. In 1938, in the Encyclopedia of Unified Science, Frank suggested that metaphysical terms do not belong in science. There he said, "A 'soulless' psychology and a 'matterless' physics have been established as parts of 'Unified Science.' Words like 'matter' and 'mind' are left to the language of everyday life where they have their legitimate place and are understood by the famous 'man in the street' unambiguously."¹⁴

¹³Cf. "The Growth of Science and the Structure of Culture: Comments on Dr. Frank's Paper," Daedalus 87, 1958, 67-76.

¹⁴Op. cit., 502.

Similarly, one critic of Frank's book, Modern Science and Its Philosophy, suggested that its main thesis was: "There are no significant questions that are so profound that they cannot be solved by the exact sciences."¹⁵

Toward the end of his life, Frank exhibited an increasing tendency to view science proper and its metaphysical interpretations to be somehow integrated in the person of the scientist. The connection is not a logical one; it is psychological, and it certainly is real. In respect to the question asked about materialism and science, then, one can claim this: Science does not lead to, or support, a materialistic view of life, nor does it discourage or refute it. Such a view of life precedes the knowledge that science offers, since it is part of the makeup of the scientist.

In order to avoid the materialistic interpretation, if it is to be avoided, educators must do two things. First, as Frank suggests, they must show the historical development of science and its several revolutions, and the metaphysical interpretations that have been attached to it from time to time. This will break down the idea, on the one hand, that there is a 'right' interpretation, and on the other hand, it will show how closely linked science and its interpretations are, as a matter of fact. Secondly, educators must establish the goals for which they wish science to be employed, not only as a technical tool, but especially, as a way of thought.

¹⁵L. N. Ridenour, Saturday Review of Literature, 7-30-49, 9.

These ideas do not tell what Frank may have felt personally with respect to materialism as a philosophy. This writer is unable to interpret his views in this regard, with any degree of certainty. One might assume, from the interest Frank had in such groups as the Conference on Science, Philosophy and Religion, that he was not a materialist, although his background was positivistic. Frank was born a Jew, which could support the same conclusion. Yet one of his lifetime friends has suggested this conclusion is on shaky ground. She says, "Philipp might have been an atheist."¹⁶

In terms of his views on truth, Frank was an empiricist. However, he seems to view empirical knowledge in a very broad way - broad enough, one might even say, to admit of all experience. In 1950, for instance, he stated,

Beyond experience, reason and imagination there are no faculties by which the human mind can find knowledge. The cooperation of these three faculties follows one and the same general pattern in all fields. Not only science proper, but also politics and religion, are ultimately based on principles that are the result of creative imagination, and that are tested by experience.¹⁷

A scientist who claims to base his ideas on facts must take into account historical and psychological facts too. Frank noted this frequently. Thus the truest view of the world must include the scientist in it as a human, alongside the things he studies.

¹⁶Letter from Mme. R. von Mises to Justin Synnestvedt, April 10, 1969.

¹⁷Relativity: A Richer Truth, 85.

In the words of Heisenberg, which Frank cited in one of his last writings, "Method and object can no longer be separated."¹⁸ Elsewhere, in the same year, Frank suggested that it is false to separate pure science from cultural values. Such a view, he maintained, "ignores the close ties which have always and everywhere existed between man's picture of the physical universe and his picture of an ideal human society."¹⁹

Finally, collating these ideas, one is safe to assume that Frank felt science does not strengthen any one view of the nature of reality over any other. Rather it is a way of symbolizing reality, just as poetry is another way. Neither way gives the 'true picture'. As Frank said, "Reality can only be experienced, never represented."²⁰

5. Science and Democracy

After World War II there were many philosophical conferences and many works written to reexamine democratic ideals, and to determine what forces in society might explain the growth of autocracies in various parts of the world. Frank belonged to one such effort - the Conference on Science, Philosophy and Religion, from its start in 1940. His book, Relativity: A Richer Truth, published in 1950, is an integration of his ideas as they were

¹⁸"Man Confronts Himself," loc. cit.

¹⁹"The Present Role of Science," loc. cit., 14.

²⁰"Contemporary Science and the Contemporary World View," loc. cit., 65.

presented and developed during the decade of his concern with that Conference. It was Frank's belief that science - or, more exactly, the 'spirit of modern science' - is in no way deleterious to the ideal of a democratic society. On the contrary, it provides the possibility for establishing that ideal in the thinking of its students with something more permanent and valid than prejudice or propaganda.

How a good science education can accomplish this is, in Frank's opinion, quite simple. What one might call the scientific attitude is one that is basically critical of new ideas until it has examined them in the light of experience. Because it tries to stay close to the 'facts', it is antimetaphysical. And because it is conscious of the need for exact 'operational definition', it is wary of oversimplifications, and the application of generalities to realms that are untested. In Frank's words, "If the student of physics gets his instruction with a view to the integration of knowledge, he will learn that science cannot 'prove' any fact. Instead, it confirms principles by the description of facts that are 'observed' by scientists generally."

In his years at Prague, during the long build-up toward the Second World War, Frank had many opportunities to see the evils that can be supported by an unscientific attitude. One example will suffice for illustration - the Nazi concept of racial superiority. Because it was useful as a propaganda tool, the Nazis took over wholeheartedly the idea of Aryan supremacy which was initiated in the nineteenth century, and strengthened by Nietzsche.

The term non-Aryan soon came to refer, under Nazi thinking, to Jews, who were used as a scapegoat for all German problems. However, when Finns, and Japanese came to be allied with the Nazis, it was suggested, in all seriousness, that they were neither Aryans, nor non-Aryans. This obvious contradiction is typical of the abuse to which language can be put, if it is viewed unscientifically.

The scientifically trained person will not be apt to follow banners or slogans blindly. These can be meaningful, if the content they symbolize is made known. But they can also become a tool for covering over specific meanings that leaders do not want to disclose. What meaning, for example, does one man give to the national flag, when he salutes it freely from love? And how does that meaning compare to the meaning understood by another man whose faith forbids him to salute, and who is prosecuted as a result?

The thoughtful person will try always to see within the symbol the specifics of the symbolism, and act accordingly. Such a "pragmatic spirit," as Frank called it, "has nothing to do with" philosophical skepticism.²¹ Rather, the meaning of principles consists, for the scientist, in the facts which follow from them, and, as Frank concluded, "not in their sound or in the pleasurable emotions this sound arouses."

²¹"Science and Democracy," in Science, Philosophy and Religion: A Symposium, Conference on Science, Philosophy and Religion, New York, 1941, 223.

CHAPTER VIII

CONCLUSION

1. Trends in Frank's Thought: Thesis, Antithesis, Synthesis

Indicated in the Preface above, the purpose of this study has been to exhibit the ideas of Philipp Frank as representing some of the major developments in the philosophy of science, to mid-century. It will be the effort of this Chapter to recapitulate briefly some of the points developed already, in order to confirm this thesis.

The first historic trend that Frank's thought mirrors is the replacing of metaphysical idealism by positivistic empiricism. The Renaissance, as Whitehead and Frank suggest, brought an autonomy to science which encouraged, first, an anti-religious reaction, and after that, an anti-metaphysical reaction too.¹ This scientific or positivistic trend, inspired by the successes in mathematics and physical science during the seventeenth and eighteenth centuries, and supported by the ideas of British empiricists, had all but molded the intellectual milieu of European scientists - including Frank and his young colleagues in Vienna. One aspect of this positivism was a strong distaste for metaphysics, which scientists considered to be vain and pompous.

¹Cf. Chapter III, section 5.

Of course the nineteenth century also witnessed the culmination of romantic idealism - primarily Kantian thought in much of Germany - and an anti-scientific view as well. However, this was not the primary influence on the young Frank, by his own admission. What it provided was an area of non-scientific thought to which Frank and his friends could turn, when they began to notice the problems which the end of mechanistic science brought about.

The second trend Frank's writing brings out is the resurgence of metaphysics once more, as a reaction to certain revolutionary developments in mathematics, logic, and physics during the nineteenth century. Frank points out that Newtonian mechanics began to collapse in the face of non-Euclidean mathematics and electro-magnetics.² This invited the feeling that science, with a capital 's', had somehow failed. Into the vacuum left by mechanism poured all sorts of religious and metaphysical interpretations of the new science, all trying to say, in effect, "I told you so." For Frank and his associates, steeped in positivistic tradition, there was a period of intense thought and study of the old and new scientific ideas, together with their interpretations, to discover whether or not science had really failed and must be abandoned. Frank shed light on some of the metaphysical misinterpretations of science, which have been discussed in this work.³

²Cf. Chapter II, section 2.

³Cf. especially Chapters IV and V.

The developments being discussed here are not clear and distinct, however, in terms of historic periods. They overlap, and at times run side-by-side in time, so that their edges are fuzzy. The distinction between these periods is further smeared because some persons tend generally toward the metaphysical viewpoint, while others tend always toward the empirical. So, too, if Frank is to be representative, we must see him as developing, not in distinct, isolated stages, but continuously. For this reason, we cannot safely say his early ideas were metaphysical, or that his middle period was given to Logical Positivism, or that in the wisdom of age he reached the sought-for synthesis although there is some truth in this simplification. Our study has tried to show that the humanism, breadth, and moderation of Frank's ideas developed along the line, throughout his life. For this reason, too, it is not often easy to discover just what Frank thought. At any one point, his thoughts were a combination of pros and cons - further complicated by his objectivity in reporting, even with respect to his own development.⁴

What saved science for Frank and his acquaintances, as we have pointed out, was the synthesis of several important ideas, from men like Mach, Poincare, Duhem and Einstein.⁵ Of course, the synthesizing itself was done mainly by Frank and his colleagues.

⁴We have shown several instances of misunderstanding on the part of Frank's critics, which may, in part at least, be excused for this reason.

⁵Cf. Chapter II, section 3.

The message they took from these four men, and others of lesser importance, had two main parts. First, it was shown in various ways that the tenets of Newtonian mechanics, which had come to be viewed as self-evident truths about observable nature were in fact only useful constructions of the mind, set up to give sensible causal form to the explanation of observed phenomena, and to provide a means for predicting future events. Mass, velocity and energy are examples of such mental constructs, which by themselves have no more claim to reality than the most unreachable ideas of metaphysics. It was shown concomitantly that traditional mechanics, which employed such terms, was neither sacrosanct, nor was it synonymous with science in general. It was one of many possible systems for viewing the world. Consequently, for Frank's group, science became free to develop along modern lines, without carrying along the burden of metaphysical trappings that had been hung on Newtonian mechanics.

2. Language Analysis: A Contribution of Modern Positivism

The second part of the synthesis we are discussing was closely related to the first, having to do with the language of science. In this area, several things are noteworthy. First, nineteenth-century advances in mathematics and logic led to the realization that there may be many different language systems to describe things, and that no one system carries any guarantee of being the true interpretation of facts. The reason for choosing one system over another - say Euclidean over Riemannian geometry -

is just a matter of its convenience and appropriateness for the job to be done. Frank pointed this out.⁶ Similarly in logic, one may choose two-valued, or multi-valued systems, depending on what he wishes to describe.

Just what constitutes the connection between a language system and what it purports to describe is another sector of the language development that concerned Frank. In scientific work, which proved to be most amenable to analysis, Bridgman presented the phase called 'operationalism', which can be summarized as follows: The meaning of a scientific term, say mass for example, does not inhere in the name. Rather, a term is properly defined simply by elucidating what operations one may perform to illustrate what the term signifies. This signification will always be relative to other terms. The complex of terms are mutually defined by the interrelation of the operations to which they refer. Mass, in our example, could mean the measured value one gets for m_1 or m_2 when they are related to other measured values by the equation:

$$F = G \frac{m_1 \cdot m_2}{r^2} .$$

However, Bridgman added, there is a danger of circularity, if one defines all the members of such a relation only in terms of the other members. One must test such operational definitions by independent means - as, for example, by other operations. In the

⁶Cf. especially Chapter VI, section 6.

example of mass, one could solve another equation involving 'm' which is independent of the first, and comparing the values by measurement. Newton's Second Law: $F = m \cdot a$, might provide such a second test.

Bridgman's developments in scientific language were incorporated in the Vienna group's emphasis on 'physicalism', which proposed to reduce scientific statements to their least common denominator, in terms of sense-data, or sense-related statements called 'protocols.' This phase of Frank's thought is epitomized by his words, "A 'soulless' psychology and a 'matterless' physics have been established as parts of 'Unified Science.' Words like 'matter' and 'mind' are left to the language of everyday life where they have their legitimate place ..."⁷

There was a similar effort to reduce all language to explicit sense-oriented statements - atomic propositions, as Wittgenstein, for one, called them. Of course, it was seen, too, that many statements can have a clearly defined meaning without depending on sense-experience. Such are the statements in mathematics and logic, like, "A straight line is the shortest distance between two points," or "Not p and -p simultaneously." This sort of statement, together with empirical statements, tied down by suitable operational definitions, were said to exhaust the realm of meaningful discourse. All else was cognitatively empty.

⁷Cf. Chapter IV, section 2, on "The Abuse of Scientific Language."

3. The Rise and Fall of Logical Positivism

Such ideas as these developed into the Logical Positivism of the Vienna Circle. Although Frank was influenced by the ideas, and added his own to the rest, he remained somewhat apart from the main movement, for reasons of practicality, as well as by choice. This has been pointed out frequently.

Ramifications of linguistic and scientific analyses propounded by the Vienna Circle led to the Analytic school, and still later to the so-called ordinary language philosophy within and without Logical Positivism - especially in England and America. The main movement, covering a period, say, from 1910 to 1940, developed many valuable contributions, as this essay has tried to point out.⁸ Nonetheless, it included some insurmountable problems. Doctrinally speaking, its major weakness lay in the so-called 'criterion of verifiability.' Put in simplest terms, this doctrine held that no statement is meaningful unless it is (1) empirically verifiable, or (2) necessarily true, in the manner of mathematical and logical propositions. The argument against this doctrine can be summarized as follows: If every meaningful statement either has logico-mathematical necessity, or else can be empirically verified, then the criterion for meaningfulness is itself meaningless, because (1) it is not logico-mathematical in form, and (2)

⁸Cf. Chapter II, section 8.

it is not empirically true, since it cannot be tested, and many people doubt it.

More important than this technical problem with Logical Positivism, however, is the attitudinal problem it symbolizes, and it is here that Frank differs most strongly from that movement. Frank, as well as Carnap and others, came to criticize the Vienna group's efforts at linguistic reduction, for going too far.⁹ Clearly there were concepts, even in the sciences, which could not be reduced to the level that Bridgman, for example, had suggested. But what the criticisms that Frank and others put forward really mean, for our purposes, is that they began to see in Logical Positivism a degree of self-certainty and oversimplification that was objectionable. It was inappropriate to face a world full of confusion and complexity with this attitude. More specifically, Logical Positivism did not allow enough room for a broad humanistic interpretation, to include areas of discourse that were emotional, psychological, religious, aesthetic and the like. According to our thesis, this is just the reason why Frank never fully embraced Logical Positivism, and why he left it more and more with time's passing.¹⁰

Such criticisms aimed against Logical Positivism for its lack of breadth and its over-emphasis of logico-mathematical

⁹Cf. Chapter II, sections 3 and 4.

¹⁰Cf. Chapter II, section 7.

thought, brings us to the culmination of this study. These criticisms point up the need for synthesis in philosophy and science, just as Frank's own development shows an effort at such synthesis. There is a dialectic pattern in his life, if one may borrow Hegelian terms. The metaphysical domination of science up to about 1600, and Frank's corresponding metaphysical interests, form a thesis; the developing autonomy of science from metaphysics - say from 1600 to 1900 - and Frank's interests in pure physics and Logical Positivism represent an antithesis; finally, current developments, which emphasize the historical and sociological aspects of science, represent a synthesis of the former extremes. But as we have suggested above, our dialectic has fuzzy edges, because it is a theoretical model forced upon the facts. It does not fit in all respects.

4. Philipp Frank: Pluralist, Humanist, Relativist

The complexity of Frank's ideas makes of him a good paradigm for contemporary philosophy of science. Just as his avowed aim was to synthesize philosophy and science, his many-faceted ideas demand from us, too, a synthesis of some sort. A brief listing of his traits may accomplish this.

Frank was an intellectual, capable of great abstraction and erudition, as illustrated by his mastery of languages, and his knowledge of Einstein's work in relativity. But he was also an activist, originating and supporting numerous agencies for science and philosophy - especially those which might help to improve

the quality of education and inspire an appreciation for democracy. Frank was a theoretical physicist, yet his concern for experimentation we have also noted. Frank was a traditionalist, insofar as he aimed at historical perspective and demonstrated his debt to people like Mach, Kant, Hume, and even Aquinas. He was also modern, praising the developments of contemporary science and methodology, and criticizing the untested metaphysics and outmoded mechanics, even of twentieth-century scientists. Frank was pragmatic, often mentioning the thoughts of James and Peirce in support of the social meaning of theories - their 'cash value'. Yet he also dealt in metaphysics - perhaps a simplified, common-sense type, but metaphysics nonetheless. Frank showed concern with moral questions, especially those surrounding the abuse of political power. Nevertheless he felt that most efforts to find a perennial moral system to serve as a guide to education - such as R. M. Hutchins had advocated -¹¹ would probably fail. In this regard, Frank said,

The trouble with such a program (as Hutchins proposed) is, of course, the problem of finding these principles of permanent validity. As a matter of fact, the permanence of the philosophical principles can be kept up and guaranteed only by spiritual or secular authorities or both. No university education can be based on metaphysics unless the choice of it is decided by an authority that is permanently in control of the teaching.¹²

¹¹Cf. Philosophy of Science, xiii.

¹²Ibid.

Note, Frank did not say it is wrong to look for metaphysical principles, to use, for example, in guiding a university curriculum; it is simply impractical to find such principles that are truly permanent. That Frank did, in fact, believe in the efficacy of guiding principles is definite. They are part - a non-scientific part, granted, but nonetheless a real part - of our given culture. To deny them would be as foolish as it would be near-sighted. Frank commented to this effect in one of his last papers, delivered to the Twelfth International Congress of Philosophy, at Venice, in 1958. He stated that the man who posits a 'strict dichotomy' between 'facts' (e.g. of science) and 'values' (e.g. of politics) "ignores the close ties which have always and everywhere existed between man's picture of the physical universe and his picture of an ideal human society."¹³

In sum, Frank was a pluralist, a humanist and always a moderate person. Faced with so many viable options, he was also a relativist. But that term must be applied cautiously in this case, and only now, after we have studied Frank, can we apply it in the same way that Frank would apply it. In his book, Relativity: A Richer Truth, Frank proposes that relativism is a necessary and useful part of our culture. He does not favor the skeptical relativism which likes to cite relativity theory to prove that nothing is certain. Rather, what Frank means is the attitude

¹³Cf. "The Present Role of Science," in Acts of the Twelfth International Congress of Philosophy, Florence, 1960.

of suspending judgment until the facts are in - the willingness to take the time to examine, scientifically and objectively as possible, all sides of the question. Finally, he means also that when one does make a judgment, he makes it in modest terms - not with self-confident aplomb - because he realizes that when our tools are more refined, or when the socio-psychological situation demands it, there will be a re-examination, and a re-evaluation, and most likely the judgment will have to be changed. In this sense of relativism, as Frank defines it, we see one of the best offspring of modern scientific thought. It is the attitude which characterizes Frank himself.

Frank was not a highly original thinker. His abilities lay more in the way of rethinking and synthesizing what had already been said. He did not have a pet theory for which he might become famous. His ideas were many, and his approach was modest. He fits well the description of Werner Heisenberg, in The Physicist's Conception of Nature, that 'modern science' (after Galileo) is characterized by 'modesty'. This modesty disappeared in the nineteenth century, but has reappeared in the twentieth.¹⁴ Perhaps such modesty will prevent Frank from being widely read; it should not, however, prevent a thoughtful student who reads him from discovering an impressive depth of knowledge, well expressed.

Frank discussed most of the aspects of philosophy of science. He shed light on all of them, primarily by refusing to lose

¹⁴Cf. "Man Confronts Himself," Frank's review of Heisenberg's book, in Saturday Review of Literature 41, Oct. 25, 1958.

sight of the whole. In this way, his contribution appears to be an eloquent reaffirmation of the Greek norm: moderation. No method or insight may claim to offer the whole truth. Frank showed that metaphysics alone is vague and ineffective; he also showed that empiricism alone is pointless. A philosopher of science - if he follows Frank's leading - will make use of both.

BIBLIOGRAPHY

1. Books by Philipp G. Frank

- 1925 Die Differentialgleichungen der Mechanik und Physik, with R. von Mises, Braunschweig.
- 1928 Relativitätsmechanik, in Handbuch für physikalische und technische Mechanik, Leipzig.
- 1932 Das Kausalgesetz und seine Grenzen, Vienna (French tr. in 1937).
- 1935 Das Ende der mechanistischen Physik, Vienna (French tr. in 1936, Czech tr. in 1938).
- 1936 La Fin de la physique mécanique, Paris (original in German).
- 1937 Le Principe de causalité et ses limites, Paris (original in German).
- 1938 Rozvrat mechanistické fyziky, Prague (original in German).
- 1938 Interpretations and Misinterpretations of Modern Physics, Paris.
- 1941 Between Physics and Philosophy, Cambridge, Mass.
- 1943 Relativity and its Astronomical Implications, Cambridge, Mass.
- 1946 Foundations of Physics, Vol. I No. 7 of the International Encyclopedia of Unified Science, Chicago.
- 1947 Einstein: His Life and Times, New York (German tr. in 1949, French tr. in 1950).
- 1949 Einstein: Leben und Zeit. Munich (original in English).
- 1949 Modern Science and its Philosophy, Cambridge, Mass. (enlarged revision of Between Physics and Philosophy).
- 1950 Einstein et son temps, Paris (original in English).
- 1950 Relativity: A Richer Truth, Boston.

- 1952 Wahrheit - relativ oder absolut, Zurich (original in English).
- 1956 (editor) The Validation of Scientific Theories (with introduction and opening article by Frank).
- 1957 Philosophy of Science: The Link Between Science and Philosophy, Englewood Cliffs, N. J.

2. Articles by Philipp G. Frank

- 1907 "Kausalgesetz und Erfahrung," Ostwald's Annalen der Naturphilosophie 6, 445-450 (tr. in Modern Science and Its Philosophy).
- 1908 "Mechanismus oder Vitalismus? Versuch einer präzisen Formulierung der Fragestellung," Ostwald's Annalen der Naturphilosophie 7, 393-409.
- 1910 "Gibt es eine absolute Bewegung?" in Wissenschaftliche Beilage zeitschrift 23, Jahresbericht der Philosophie Gesellschaft am den Universite Wien.
- 1911 "Das Relativitätsprinzip und die Darstellung der physikalischen Erscheinungen im vierdimensionalen Raum," Ostwald's Annalen der Naturphilosophie 10, 129-161.
- 1917 "Die Bedeutung der physikalischen Erkenntnistheorie Machs für das Geistesleben der Gegenwart," Naturwissenschaften 5, 65-72 (tr. in Modern Science and Its Philosophy).
- 1918 "Josef Popper-Lynkeus zu seinem achtzigsten Geburtstage," Physikalische Zeitschrift 19, 57-59.
- 1919 "Die statistische Betrachtungsweise in der Physik," Naturwissenschaften 7, 701-740.
- 1920 "Wissenschaft und Theologie," Freier Gedanke, Nr. 40.
- 1926 "Theologische Elemente in den exacten Wissenschaften," Prager Tagblatt, December 8.
- 1928 "Über die Anschaulichkeit physikalischer Theorien," Naturwissenschaften 16, 122-128.
- 1928 "Gibt es ein irrationales Moment in den Theorien der moderne Physik?" N. Zurich Zeitung, December 17.

- 1929 "Was bedurten die gegenwertigen physikalischen Theorien fur die allgemeine Erkenntnislehre?" Naturwissenschaften 17 (tr. in Modern Science and Its Philosophy).
- 1929 Reprint of above, in Erkenntnis 1, 126 ff.
- 1931 "Der Charakter der hautigen physikalischen Theorien," Scientia 49, 183-196.
- 1934 "La Physiques contemporaine manifeste-t-elle une tendance a reintegrer un element psychique?" Revue de Synthese 8, 133 ff.
- 1934 "Theorie de la connaissance et physique moderne," Paris expanded version of 1929).
- 1934 "Hans Hahn," Erkenntnis 4, 315-316.
- 1935 "Die Prager Vorkonferenz '34'," Erkenntnis 5, 3-5.
- 1935 "Zeigt sich in der modernen Physik ein Zug zu einer spirituellen?" Erkenntnis 5, 65-80.
- 1935 "Jordan und der Radikale Positivismus," Erkenntnis 5, 184.
- 1935 "Positivistische oder metaphysische Auffassung der Physik?" Scientia 58.
- 1935 "Conception Positiviste ou Metaphysique de la physique?" Scientia 58, 1-9 (French translation of above).
- 1936 "Le fosse entre la physique et la biologie," Revue de Synthese 12, 51-60.
- 1936 "Logisierender Empirismus in der Philosophie der U. S. S. R." in Actes du congres international de philosophie scientifique, Paris, 1935, Vol. VIII, 68-76 (tr. in Modern Science and Its Philosophy).
- 1936 "Philosophische Deutungen und Missdeutungen der Quantentheorie," Erkenntnis 6, 303-317 (tr. in Modern Science and Its Philosophy).
- 1937 "The Mechanical Versus the Mathematical Conception of Nature," Philosophy of Science 4, 41-74 (also in Modern Science and Its Philosophy).
- 1937 "Was versteht der Physiker unter der 'Grosse' eines Korpers?" Theoria 3, 76-89.

- 1937 "La Physique moderne a-t-elle deplace frontiere de l'objet et sujet?" Congres Descartes VII, 84-89.
- 1938 "Bemerkungen zu Ernst Cassirer: Determinismus und Indeterminismus in der modernen Physik," Theoria 4 (tr. in Modern Science and Its Philosophy).
- 1938 "Die Wissenschaftlich Sprache," Erkenntnis 7, 303-422 (summary of Fourth International Congress for the Unity of Science, Cambridge, England, 1938).
- 1938 "Physik und logischer Empirismus," Erkenntnis 7, 297-301.
- 1938 "Ernst Mach - The Centenary of His Birth," Erkenntnis 7 (also in Modern Science and Its Philosophy).
- 1939 "Modern physics and common sense," Scripta Mathematica 6 (also in Modern Science and Its Philosophy).
- 1941 "Why do scientists and philosophers so often disagree about the merits of a new theory?" Review of Modern Physics 13, 171-175 (also in Philosophy of Science: The Link Between Science and Philosophy).
- 1941 "Is the Universe Infinite?" Popular Astronomy 49, No. 8.
- 1941 "Science and Democracy," in Science, Philosophy and Religion: A Symposium, Conference on Science, Philosophy and Religion, New York, 215-228.
- 1942 "Concerning an Interpretation of Positivism, Isis 33, June, 683-687.
- 1942 "The Influence of an 'uneven' anistropy on the path of light rays," Physical Review 62, 241-243.
- 1944 "The Philosophical meaning of the Copernican revolution," Proceedings of the American Philosophical Society 87, 241-243.
- 1946 "The relativity of truth and the advance of knowledge," Analisi (expanded form in Relativity: A Richer Truth).
- 1946 "Science Teaching and the Humanities," ETC 4, 3-24 (also in Modern Science and Its Philosophy).
- 1947 "The place of the philosophy of science in the curriculum of the physics student," American Journal of Physics 15, 202-218 (also in Modern Science and Its Philosophy).

- 1947 "The Institute for the Unity of Science; Its Background and Purpose," Synthese 6 (Sweden), 160-167.
- 1948 "Logical Empiricism I: The Problem of Physical Reality," Synthese 7, 457-466.
- 1948 "In Memoriam, William Malisoff," Philosophy of Science 15, 1-2.
- 1948 "The place of logic and metaphysics in the advancement of modern science," Philosophy of Science 15, 275-286 (also in Modern Science and Its Philosophy).
- 1949 "Einstein, Mach and Logical Positivism," in Albert Einstein: Philosopher-Scientist, ed. P. A. Schilpp, 271-286.
- 1949 "Einstein's philosophy of science," Review of Modern Physics 21, 349-355.
- 1949 "Zum begriff der Wertschaftsordnung," Die Neue Ordnung 3, 521-529.
- 1949 "Relativity, Truth and Values," in Perspective in a Troubled Decade, ed. L. Bryson, Columbia University, 203-214.
- 1950 "Our Attitude Towards All Questions," Saturday Review of Literature 33, April 8, 26.
- 1950 "Science Linked to Life," Saturday Review of Literature 33, May 6, 25-26.
- 1950 "Comments on realistic versus phenomenalist interpretations," Philosophy of Science 17, 166-169.
- 1950 "Introduction to the philosophy of physical science on the basis of logical empiricism," Synthese 8, 28-45.
- 1950 "Metaphysical Interpretations of Science," British Journal for the Philosophy of Science 1, 60-91.
- 1951 "The logical and sociological aspects of science," Proceedings of the American Academy of Arts and Sciences (Daedalus) 80, 16-30.
- 1952 "The origin of the separation between science and philosophy," Proceedings of the American Academy of Arts and Sciences (Daedalus) 80, 115-139 (also in Philosophy of Science: The Link Between Science and Philosophy).

- 1952 "Non-scientific symbols in science," in Symbols and Values: An Initial Study, ed. L. Bryson, New York.
- 1953 "The role of authority in the interpretation of science," in Freedom and Authority in Our Time, ed. L. Bryson, New York, 361-363.
- 1954 "Conference on Validation of Scientific Theories," Science 119, February 19, 233-234.
- 1954 "Work of Richard von Mises: 1883-1953," Science 119, June 11, 823-824.
- 1954 "The variety of reasons for the acceptance of scientific theories," Science 119, (also in The Validation of Scientific Theories, ed. P. Frank, 3-17).
- 1957 "Philosophical Uses of Science," Bulletin of Atomic Science 13, April, 125-130.
- 1958 "Man Confronts Himself," Saturday Review of Literature 41, October 25, 20-21.
- 1958 "Contemporary science and the contemporary world view," Daedalus 87, 57-66.
- 1958 "The Present Role of Science," in Atti Del XII Congresso Internazionale di Filosofia, Sansoni, Firenze, 1960, Given in Venice, September 12-18, 1958 (also in The Humanist 19:1, 1959, 3-13).
- 1963 "The pragmatic component in Carnaps's 'Elimination of metaphysics'," in The Philosophy of Rudolf Carnap, ed. P. A. Schilpp, Chicago, 159-164.

3. Commentaries on Philipp G. Frank and His Work

- 1936 Portrait, Musician 41, March, 64.
- 1936 P. Lecompte du Nouy, "Zum Vortrag Franks," Erkenntnis 6, 339-340.
- 1936 F. Gonseth, "Zum Vortrag Franks," Erkenntnis 6, 340-342.
- 1937 G. Oxenstierna, "Comments on Frank's 'Was Versteht der 'Grosse' eines Körpers?'," Theoria 3, Gothenburg, 90-114.
- 1941 L. S. Feuer, "Review of Between Physics and Philosophy," Science and Society 5:3, 222-233.

- 1943 Review of Between Physics and Philosophy, Nature 152, October 9, 397-398.
- 1947 L. S. Feuer, "Review of Einstein: His Life and Times, Science and Society 11;3, 259-270.
- 1948 McVittie, "Review of Einstein: His Life and Times, Nature 162, October 16, 591.
- 1949 L. N. Ridenour, Review of Frank's Modern Science and Its Philosophy, Saturday Review of Literature 32, July 30, 9.
- 1949 R. G., Biographic Sketch of Philipp Frank, Saturday Review of Literature 32, July 30, 9 (article initialed).
- 1951 O. Potter, "Note on Philipp Frank's Interpretation of Science," British Journal for the Philosophy of Science 2, 58-60.
- 1951 J. H. Woodger, "Review of Modern Science and Its Philosophy," British Journal for the Philosophy of Science 2, 168-169.
- 1956 G. J. Holton, "Science and the Modern World View," Science 124, August 17, 327-328.
- 1958 R. Oppenheimer, "Comment of Dr. Frank's paper," Daedalus 87, 67-76.
- 1958 C. W. Kegley, "Philipp Frank on Scientific Theory," in Acts of the Twelfth International Congress of Philosophy, Venice, September, 1958, 231-237.
- 1960 F. J. Rutherford, "Frank's Philosophy of Science Revisited," Philosophy of Science 27, 183-186.
- 1965 P. G. Bergmann, R. Carnap, R. Furth, G. J. Holton, E. C. Kemble, H. Margenau, H. von Mises, E. Nagel, R. J. Seeger, K. Sitte, "Greetings to Philipp Frank," in Boston Studies in the Philosophy of Science, Vol. II, ed. Cohen and Wartofsky, N. Y., 1965, vii-xxx.
- 1965 "Bibliography of Philipp Frank," in Boston Studies in the Philosophy of Science, Vol. II, ed. Cohen and Wartofsky, xxxi-xxxiv.
- 1966 Obituary, New York Times, July 3,
- 1966 "Philipp Frank Dies at 82," Science News 90, August 6, 84.

- 1966 G. Holton, P. G. Bergmann, L. Tisza, E. Nagel, J. T. Clark, E. C. Kemble, J. Bernstein, R. S. Cohen, "Expressions of Appreciation as arranged in the order given at the memorial meeting for Philipp Frank, October 25, 1966, at the Memorial Church of Harvard University" (No publisher or date of publication on pamphlet).
- 1967 Obituary, Encyclopedia Britannica Book of the Year, 592.
- 1968 G. H. Holton, E. C. Kemble, W. V. Quine, S. S. Stevens, M. G. White, "In Memory of Philipp Frank," Philosophy of Science 35, March 1-5.
- 1969 Letter from M^{rs}. R. von Mises, to Justin Synnestvedt, April 10.

4. Works Relevant to This Study

- Alexander, P., "The Philosophy of Science 1850-1910" in A Critical History of Western Philosophy, ed. D. J. O'Connor, Free Press, N.Y., 1964.
- Ashby, R. W., "Logical Positivism," in A Critical History of Western Philosophy, ed. D. J. O'Connor, Free Press, N.Y., 1964.
- Britton, K. W., "Logical Positivism," in Encyclopedia Britannica, 1966 ed.
- Cohen, R. S. and Wartofsky, M. W., ed. Boston Studies in the Philosophy of Science, Vol. II, Humanities Press, New York, 1965.
- Coleman, J. A., Relativity for the Layman, Signet Science, New York, 1954.
- Danto, A. C. and Harre, R., "The Philosophy of Science," in Encyclopedia of Philosophy, Vol. VI, 289-300, ed. Paul Edwards, McMillan Co., and Free Press, New York, 1953.
- Feigl, H., and Brodbeck, M., ed. Readings in the Philosophy of Science, Appleton-Century-Crofts, New York, 1953.
- Hartnack, J., Wittgenstein and Modern Philosophy, transl. M. Cranston, Doubleday, New York, 1965.
- Hill, T. E., Contemporary Theories of Knowledge, Ronald Press, New York, 1961.
- Katz, R., An Introduction to the Special Theory of Relativity, Van Nostrand, Princeton, 1964.

- Kraft, V., The Vienna Circle, transl. A. Pap, Philosophical Library, New York, 1953.
- Marks, R. W., ed. Space, Time and the New Mathematics, Bantam, New York, 1964.
- Neurath, O., Carnap, R., and Morris, C., ed. International Encyclopedia of Unified Science, Chicago University, 1938.
- Pap, A., An Introduction to the Philosophy of Science, The Free Press, New York, 1962.
- Schilpp, P. A., ed. Albert Einstein: Philosopher-Scientist, Tudor, New York, 1949.
- Schilpp, P. A., ed. The Philosophy of Rudolf Carnap, Open Court, La Salle, Illinois, 1963.
- Smith, E. P., Muzzey, D. S., and Lloyd, M., World History: The Struggle for Civilization, Ginn and Co., 355-387.
- Urmson, J. O., Philosophical Analysis, Oxford, England, 1956.

APPROVAL SHEET

The thesis submitted by Justin Synnestvedt has been read and approved by members of the Department of Philosophy.

The final copies have been examined by the director of the thesis, and the signature below verifies the fact that any necessary changes have been incorporated and that the thesis is now given final approval with reference to content and form.

The thesis is therefore accepted in partial fulfillment of the requirements for the degree of Master of Arts.

4/10/70
Date

Edward A. Mager
Signature of Advisor